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THE

WESTERN JOURNAL

OF

MEDICINE AND SURGERY.

EDITORS.

LUNSFORD P. YANDELL, M. D.

PROFESSOR OF PHYSIOLOGY AND PATHOLOGICAL ANATOMY

IN THE UNIVERSITY OF LOUISVILLE.

AND

THEODORE S. BELL, M.D.

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THE

WESTERN JOURNAL

OF

MEDICINE AND SURGERY.

JULY, 1852.

Art. I.—*Water as a Remedy in Disease.* By John C. Darby, M. D., of Lexington, Ky.

My attention was first called to the use of cold water in fevers, externally applied, by Dr. Cooke, Professor of the Theory and Practice of Medicine in Transylvania University, while I was a student in that institution. Dr. Cooke strongly recommended the practice taught by Dr. Currie. I have since several times read Dr. Cooke's Review of Currie's Medical Reports. At the same time we were very fully instructed by Dr. B. W. Dudley, as to the use of warm water as a local application or bath in almost all forms of inflammation. But I did not begin to prescribe the cold affusion or cold bath in fevers to any extent, till about the year 1845; when my attention was again particularly called to the subject by notices in newspapers of

the *Hydropathic Practice.* I again read Currie, and such works on Hydropathy as I could obtain. In 1847, being in bad health, I went to New England to recruit, and to see that country, and took the opportunity to visit Brattleboro, Vermont, and examine into the water-cure system for myself. Dr. Wesselhoeft, the very gentlemanly proprietor, received me with great kindness, and treated me with marked courtesy. There were from seventy-five to eighty patients under treatment. I was introduced to nearly all of them, and as it was a rare thing for a "regular doctor" to visit such an establishment, except to find fault and to criticise, and as I gave them all every assurance that such was not my purpose, I had every opportunity afforded me for examination and investigation. I inquired particularly into the history of almost every case; and indeed the patients seemed to take pleasure in having me listen to their past experience as invalids. I passed the time very pleasantly, and learned a good deal; and shall always remember my sojourn of ten days at Brattleboro, as among the most pleasant of my life—I would say, in passing, that all invalids who have made up their minds to try this practice, will, in my opinion, be pleased with Brattleboro. Dr. Wesselhoeft is a man of good sense, well informed in his system; and I believe him to be an honest man and a gentleman; and have no doubt that he will always give a candid opinion to all who consult him. As to the establishment, I can hardly conceive of one being more thoroughly adapted to the purpose. There is an abundant supply of cold freestone water, and the various baths are all admirably arranged.

What I have said above, I feel to be due for the kindness and marked civility with which I was received and treated at Dr. Wesselhoeft's establishment.

I was not in any sense a patient for water treatment, and so informed Dr. Wesselhoeft. But as I went there for information, I asked him to allow me to try his various

baths; which he readily consented to, and for which he made no charge—my object was to ascertain how far cold ablutions could be safely practiced *when the temperature of the body was not above the standard of health.* Dr. Currie had taught us how to use water in fevers. He says, "the affusion of cold water may be applied in a case of fever when there is no sense of chilliness present, when the heat of the surface is steadily above what is natural, and when there is no general or profuse perspiration." Dr. Currie, it seemed to me, had laid down and clearly established the rules to be observed in the use of cold water *in fevers*—I desired to study the other side of the question;—the use of cold affusions when there was no unnatural heat, and in other complaints besides fever.

I concluded that, if I would submit to the most powerful baths, so far as safety was concerned, they would include the less powerful. I was packed in the wet sheet three times; I took the running sits (from a cold spring) three times, and stood under the highest and largest douche twice. I found them all not only safe, but delightful and invigorating—I did not try the dripping sheet, nor did I apply a wet compress. I submitted while there to the regimen of the establishment, and at the expiration of the time (ten days) I found myself much improved in strength, and in a feeling of well-being.

It is unnecessary to give a description of the various forms of Hydropathic baths; it is to be presumed that nearly every reading man in the country understands what is meant by "packing in the wet sheet," "sits bath," "douche," "dripping sheet," &c. &c. The general conclusion to which I came as to the water-cure system was this; that it is admirably adapted to the treatment of many varieties of chronic disease, such as rheumatism, dyspepsia, piles, chronic bowel complaints, neuralgia, and that form of ill health which is the result of hard study or long continued and close confinement to business in

cities; cases in which a close diagnosis would show that there was no particular organ diseased. I speak now of Hydropathy as practiced in an establishment; I will have something to say of some of the appliances of Preissnitz in common-out-of-door practice, after a while. I suppose that the cases of chronic diseases which, with the above view, properly come under the Hydropathic physician, must be treated in *an establishment*. To make it the system and practice of Priessnitz, various baths have to be prepared; requiring, in the first place, an inexhaustible supply of water; and the water must be cold, and soft or free-stone water; moreover, as other laws of Hygiene are made to form a part of the system, the establishment ought to be in the country, or in villages, where the patients can take their daily walks through pleasant groves, away from the noise, and bustle, and dust, and heat of a city.

The first effect of the treatment is decidedly tonic. So far as I could learn at Brattleboro, there was very rarely a patient who was not delighted with the practice for the first few weeks; because it made them feel stronger and more cheerful, with a better appetite and more refreshing sleep. But after a while they begin to be depressed; the treatment is taking a stronger and deeper hold upon them, and they now become discouraged. This condition lasts for a longer or shorter time in different individuals until what is called the crisis comes. This crisis consists in the breaking out upon some part of the body of a rash, or of boils and various forms of cutaneous eruption. The patients generally improve when these occur, or at any rate they feel better, except the annoyance from the boils or eruption. But a majority of the cases are not so fortunate as to be relieved when the crisis appears: they get better for a time, and then fall back again; and perhaps it may be many months before they are cured. My opinion is, that the practice, as it is commonly pursued in

Hydropathic establishments, i. e. to be packed in the wet sheet at day-light in the morning, with its accompaniments, sweating, plunge into a cold bath, &c.; then a sits bath about 10 or 11 o'clock, A. M., or a douche, or both; to be followed by another sits bath in the afternoon; and to wind up with a foot bath at night, is too much, to be continued for any great length of time; that in chronic cases, after it has been pursued for three or four weeks, or at farthest five or six weeks, there ought to be an intermission; that then the patient should be required to take the usual amount of exercise, and to live upon the prescribed regimen [which is always good enough] for a week or two; and that so far as water is concerned it should be limited to washing the body every morning, or the dripping sheet if you choose. Chronic diseases, to be treated upon the water-cure system, must be treated in an establishment especially fitted up for the purpose. Hydropathic doctors have an advantage over regular practitioners; their patients belong almost all to the better class of society; they are persons who have ample means, and who can afford to leave home and be absent for months at a time at a heavy expense. It is the interest of the Hydropathic doctors to have the patients remain at the establishment for as long a time as possible; and they seem to be justified in requiring them to remain until their health is fully restored; but if the patients would consult their own advantage, they would take a week or two's recreation during a season, and travel about, or visit some mineral spring, for change of scene and of society, if for nothing else.

The application of cold water as a daily bath may be used with comfort and pleasure through a long life-time; but to use it as a remedy in health, i. e. as it is prescribed for invalids, would be as injurious after a certain time as if a man were to take 20 grains of calomel every day.

Some impostor in Ohio, a few years ago, started the

humbug of "tomato pills," as a substitute for calomel, and they were sold by a druggist in Lexington. I answered the imposition at the time by simply stating the fact, that tomatos were known to be a wholsome article of food was conclusive proof that they did not possess any of the qualities, powers, or properties of calomel; and so with the water-cure system: as I remarked above in respect to one class of the patients who are benefitted by water treatment, they have no particular organ diseased; all that is the matter with them, is, that their whole system is relaxed, and worn down by excessive labor, either bodily or mental, and many of them would be as certainly cured by a tour over the western prairies, or a sojourn at the sea side, or at some of the mineral springs as at a water-cure establishment.

When a man starts a new system, either in education, in medicine, in religion, or any thing else, he is apt to forget how long the world has done without him and his system. Fortunately for the human family, all knowledge does not centre in the brain of one man; nor is it confined to any race or nation.

I am also satisfied that medicines ought not to be excluded from the treatment of patients in Hydropathic establishments. I have demonstrated to my own satisfaction, that water, as directed to be used as a remedy in disease by Currie and by the Hydropathists, is not incompatible with the use of medicines or *drugs* in the treatment of *acute disease.* This I will illustrate presently by giving some cases. And from what I saw at Brattleboro, and from the facts I have met with in Hydropathic books, I am certain that, many of their patients laboring under chronic affections would be much more speedily cured, if they were permitted to take some such medicine as a good physician would prescribe for them; and that some would be cured, who, as it is, are not eured at all.

The philosophy of health and of disease consists in

part, in a knowledge of the influence of the physical agents, by which we are surrounded, upon man's animal being.

It is the province of the physician to understand those general laws, and to keep them in view when called to the bed-side of a patient. Little benefit can be expected from medicines proper, unless the physician is governed by these great laws. It is a truism, which must be admitted by all candid and thinking men, that whatever may fairly be said *to cure* a patient, (using the term in its common acceptation), must do so in accordance with the laws of the animal economy, whether the man who prescribes understands those laws or not. The allowances and qualifications to this principle are these: 1st. The patient might have got well without *the* remedy, or without *any* remedy. 2d. He may have recovered in spite of the disease and remedy together. But yet there are cases where there can be no doubt, that the individual's life was preserved or saved by the remedy. I will give one case, by way of illustration. Several years ago, I treated an old lady for (as I believe) inter-susception. She had been vomiting for nearly four days; the matter thrown up appeared to be fecal; the pain was local and severe. When I first saw her, I gave an emetic, believing the disease to be colic; after that I gave nothing but opium in some form to relieve the pain and to quiet the stomach, and I used injections of warm water constantly and freely. But the patient grew regularly worse, and finally was almost exhausted. Happening, when I called during the fourth day, to have a galvanic battery in my buggy, I concluded to try it. It was applied by an individual, who held one of the knobs, placing the other hand over the bowels of the patient, and moving it about for fifteen or twenty minutes—it acted like a charm, the bowels acted promptly and freely, and the patient was entirely relieved, and she was soon in her usual health. Now I say that

this patient would have died, in all reasonable probability, if the battery had not been used. The remedy must have acted in accordance with the laws of the animal economy, and yet I have no idea how it did act. I have selected the above case, because it does not properly come under the head of any particular system of medical practice. It illustrates the principle, for which I am contending. The fact that I have not been able to find out how the battery acted, does not affect the principle.

Well, it may be asked, am I writing to defend the Hydropathic practice? I answer that I am not. But I maintain, that the Hydropathic practice, as such, is worthy of the investigation of the regular faculty, and demands it. I maintain that it has developed and is constantly developing some important principles; and I believe that there are many cases, especially such as those referred to, such as have no special malady, but are worn down by excessive labor, mental or bodily, who ought to be recommended by their physicians to try the practice. The medical profession have perhaps benefitted the world as much by teaching mankind the laws of Hygiene, and the simple means of relieving disease in families, as they have ever been able to do at the bed-side. Whatever promises to preserve the health of man and to prolong his life, commends itself to the medical profession.

The practice of Currie had become almost obsolete when the practice of Preissnitz became known. The world is under many obligations to Preissnitz for reviving the practice of Currie, if for nothing else.

M. Louis says, in the conclusion of his volume upon fever, that bleeding and blistering appeared to have afforded more relief than any other remedies tried by him. I think that he gives the history of forty-seven cases that died. The most remarkable symptom, or one of the most remarkable and constant symptoms in these cases, was the heat of the skin. They were typhoid cases. Now, I

maintain that, had those patients been bathed according to the rules laid down by Currie, when their bodies were burning hot, M. Louis would not have had to express a doubt in his concluding remarks as to whether any remedy had been of any avail. What is fever? I answer that it is the generation of animal heat by the lungs so rapidly as to interfere with, or prevent the healthy performance of all the other functions of the body. It involves an increased action of the heart, but this is compatible with health; as when we take active exercise, or when the heart is excited by mental emotions. The chief element of fever is the continued, rapid generation of animal heat by the lungs; so as, among its first effects, to prevent the stomach from doing its office; it rejects food for the most part, or if the food is received, it is not well digested. There being then no supply of new matter to furnish fuel for this increased generation of heat, or combustion of oxygen; or to supply the waste of the body, the patient's body is necessarily reduced; and if there is a tendency to degeneration of the bowels, the glands and the mucous membrane become implicated. Now, I maintain that bathing the body with cold water, time and again, as may be necessary according to the rules of Currie; or the dripping sheet bath, according to Preissnitz, is a more rational and a more scientific practice than to bleed or to blister.

It is not necessary to discuss the *modus operandi* of the cold water practice in fever. To do justice to the question would require many pages. Suffice it to say, that the body is rapidly cooled; that the action of the heart is tranquillised; in a word, that, for the time being, the fever is broken, and the nervous system is so much invigorated, that the chances are always in favor of the fever's not returning; or if it does return, it will not be so high. Dr. Currie directs that the cold affusion should be resorted to as soon as possible, after the fever is fully devel-

oped; during the first hot stage if you can. Since 1847, I have treated many cases after this plan. But I also give medicines. I give calomel without any fear of salivation, and I have never seen a case where the gums were touched, in which I could say that it was in any way attributable to the water. I do not confine the practice to fever cases. Whenever called to a patient, whose skin is hotter than natural, with an excited pulse, and especially if the head be hot and painful, I either order the cold affusion; or that the patient be well sponged with cold water, let the disease be what it may. I rarely ever bleed in idiopathic fever; where it is complicated with inflammation I may bleed, or give an emetic; and there are cases in which I may not deem it necessary to use the cold water. The cold water practice of Currie, and even that of Preissnitz rationally understood, is not at all incompatible with the use of emetics, cathartics, or any of the ordinary methods of practice. Where there is an establishment with all the appliances of the water-cure system, I have no doubt that many cases of acute, as well as of chronic disease, may be successfully treated without medicines. But in ordinary practice, in town and country, where a physician may or may not have any convenience, he cannot, if he would, confine himself to water alone. Physicians need not fear that their practice will be injured by Hydropathy. If they will study Currie's Medical Reports, as old as they are, and study the Hydropathic practice, they will find themselves much better qualified to treat disease, than if they implicitly follow such authorities as Louis and Bouillaud. Too many lives are daily sacrificed to science "falsely so called." The study of anatomy, physiology and pathology can never be less important than they are at the present time; no discovery, however brilliant, can ever do away with a thorough practical acquaintance with the various branches of medical science; and it will always be a matter of the first importance to be able to

make a correct diagnosis. But while all this is true, it still remains a fact, that the great principles of physiology are too much lost sight of; the influence of the physical agents by which we are always surrounded upon our bodies, in health and in disease, is too little attended to. Water is one of the most common, and one of the most useful of all those agents. It may be modified in its application almost indefinitely. Let the profession, then, return to it again and give it a fair trial; and it will turn out, I doubt not, as it did when Currie published the first edition of his work; from one end of the land to the other, there will come up the reports of physicians, testifying to its great value as a remedy in fevers and other diseases.

Case I. Shortly after my return from Brattleboro, in August, 1847, I was called to see a patient, whose malady was what I would call old fashioned bilious fever. He was a large athletic man between twenty-five and thirty years of age. The fever was very high; his skin excessively hot; his pulse strong, full, and frequent. I ordered him to be undressed, and then to lie down on the floor, and as there was a spring under his room, we had the water brought up, bucketful after bucketful, until I had dashed over him some five or six. We then rubbed him dry quickly, and put him to bed. He immediately fell asleep and did well for some hours; but the fever rose the next day at the usual hour; and I again repeated the cold affusion; but as the floor of his room was covered with water, when I first bathed him, and as his wife was his only attendant and nurse, I made him go into the yard the next time, (his room was on the ground floor, and he had only to step out of the door to be in the yard), and then poured as many buckets as before over him. The fever was again removed for the time being; and again he went into a comfortable sleep. I bathed this man some five times in the same way on succeeding days, and had no occasion to repeat it afterwards. I gave such medi-

cines as I thought suited his case; among which was calomel. He was not salivated, and by the end of ten days was entirely convalescent.

Case II. Soon after the father recovered one of his sons was taken with fever, which seemed to be of the same character. I visited this boy, a child of seven or eight years of age, very seldom; for his mother, a very sensible woman, told me that she could manage his case herself;—that whenever the fever rose, she made him undress, and go into the yard, and lie down, where she poured three or four bucketsful of water from the spring over him, and then rubbed him dry and put him to bed. His fever did not last longer than that of his father. I state the case of the boy to show that when the practice has been once tried in a family, although there may be great opposition at first, it soon gives way, and the family become so much pleased with the remedy, that they have no hesitation in trying it on all occasions.

Case III. The next case in order, was a complicated case of acute rheumatism. I was called to see this man on the 16th of January, 1849. During this winter I saw nine cases of partial paralysis; it was for the most part, in my practice, complicated with rheumatism; and as I learned from other physicians that they had many cases of a similar nature, I concluded at the time that paralysis was then an epidemic. I lost no case so affected. The medicines used were such as I employed in the case of the man, a history of whose disease I am about to relate. He was a large and portly man, about fifty years of age; red hair, and thoracic temperament; a blacksmith by trade. He told me that he had been ill for a week; that he had had no physician. Some twenty years before he had had a severe attack of rheumatism, which had confined him to his bed several months; that on the present occasion he had not slept at all for a week, and could not use either arm or leg. He was paralysed. When I placed my fin-

gers on his wrist to feel his pulse, he said it gave him great pain; that he could not bear to be touched. I found his pulse strong and full, his skin very hot, and his tongue coated. The patient was immediately placed in half a hogshead of cold water, and as it had been standing out of doors, it had some ice in it; so he was washed all over with ice-water. After rubbing him dry and putting him to bed, I bandaged each arm, first with a wide bandage wet with cold water, and then covered this with a dry bandage. That night he slept well. He was also given cathartic medicine, and put upon a solution of the nitrate of potash, (ʒj to the quart of water), as a drink. The next morning when I visited him, he informed me that he had again been bathed in the tub, but that when he got into the water he found it uncomfortable, the water being too warm; his friends fearing the effect of very cold water, had carefully removed all the ice, and filled the tub up with water just from the spring. He required them to go out and get some ice and snow, and put it into the water. The cold bathing was continued from day to day for about a week; he took such medicines as I thought were indicated, but I put more reliance upon the cold bathing, and the wet bandages around the arms than upon any thing else; I dismissed the case as entirely convalescent on the 28th. It was some weeks before he recovered his strength, but he had no backset, and became, in a month or two, as strong and well as ever, and now boasts of his recovery under the water practice.

Case IV. *A Case of Croup.*—Patient, a little boy between four and five years of age—I have never seen a more marked case of croup than this. I first tried to vomit the child with ipecacuan and tartar emetic, but effected nothing; the emetic medicine was rejected by the time it was swallowed, without relieving the patient in the least. I was called to see the child at 2 o'clock, A. M., and it was now 4; finding that my little patient was

growing rapidly worse, I determined to try the wet compress. I wet a large towel in ice water, and folded it well over the chest and around the neck, and put a dry cloth over it. I also gave a decided dose of calomel. I then gave directions to renew the under wet cloth, as often as it might become hot. But before I thought it prudent to leave, the child had gone to sleep, and breathed with perfect ease. As this was during the prevalence of cholera in Lexington, and as I was very busy, I did not see the patient again until 9 o'clock, A. M. I found him sitting at a little table, eating broiled chicken and bread. He said he was well; which was the fact. There was no further difficulty. The child was well. The same treatment was used under my advice this summer, the case was croupy catarrh; and the child, a boy of seven or eight years of age, had been ill for four days. The remedy was altogether successful. The mother of the child was very much opposed to the application of the wet towel, but was much gratified at the result.

Case V. July 11th, 1849, I was called to see a young man about twenty-five years of age, who had the cholera. I found him cold; his body and limbs felt cold, and he complained very much of feeling cold. His skin was moist, but the perspiration was not excessive, nor did it ever become so. The discharges were rice water. They were not very large, though frequent. I gave him brandy, mixed with ice water, freely. He took a dose of calomel every four hours, until the discharges became bilious, and after that, the interval between the doses was sometimes increased. But I could not by any means bring about a re-action. I tried various stimulants, besides external applications, but all to no purpose. He continued in this condition for three days and nights. On the morning of the fourth day, I determined to try the cold dash. He was laid upon a sheet spread on the floor, and I then poured nearly a barrel of fresh cold water over him. He

was then quickly rubbed dry and placed between blankets in bed. Re-action commenced in a very short time, and we had no farther difficulty. He was very slightly salivated. This young gentleman has enjoyed better health since he had the cholera than he had done before. I consider it all-important, when cold water is used by affusion, that there be assistants upon whom reliance can be placed, to rub the patient dry, and to do it quickly. The brisk rubbing is absolutely necessary when the patient is cold.

I have given six cases, in three of which the temperature of the body was above the standard of health. In the croup case the temperature of the body was not above the healthy standard; in the case of croupy catarrh there was a slight fever. I have not specified particularly the medicines used in these cases. I do not consider it necessary, because my intention is merely to show, that cold water may be safely used in certain cases; and that it does not interfere with the use of medicines, but assists them; that calomel may be given with as much safety when we are using cold water, as if it was not employed at all. As I have already remarked, I consider the practice recommended by Dr. Currie particularly adapted to the fevers of the south and west; I mean miasmatic fevers, intermittent and remittent, where the re-action is excessive. I rely upon the cold affusion almost altogether to subdue the fevers of children. I prefer it to bleeding or emetics. I have the child undressed, quickly washed all over, and then wiped dry, and put to bed. I also frequently require my patients to wear a wet compress. I have afforded great relief in cases where there was a painful state of the bowels, by applying a wet compress over the bowels, with a dry one over the wet one, after the fashion of the hydropathists. The patients wear these compresses continually for weeks, day and night. The under cloth has to be re-wet whenever it becomes too hot, or begins to get dry. The outer cloth is necessary

to restrain evaporation, and to prevent the clothes of the patient from becoming damp. In one case of asthma, this wet compress has proved of very great service. The patient was a single lady, about thirty-five years of age; she had been afflicted with asthma from childhood; had tried a great many remedies with little or no relief; the attacks were frequent and very severe. I was called to see her when she appeared to be dangerously ill, and immediately applied the wet compress, using ice-water. The wet cloth was several folds in thickness, and covered the entire chest; it was covered with a dry cloth. The relief was almost instantaneous. It has been several years since I first prescribed for this lady. She has had only two or three severe attacks since I first saw her; and it has been very seldom that she has been troubled at all. She says she had never expected to enjoy such health. I believe that she still wears the wet compress. I know that she did wear it, with occasional intermissions for nearly two years. In the asthma cases that I have treated, during the spell, I have always found it necessary to re-wet the cloth as often as it began to get warm; like the application of cold water to a burn, it only affords relief while the water is cold; the pain in the one case, and the difficulty of breathing in the other, return as soon as the water begins to get warm. But between the paroxysms, the wet compress need not be changed, unless it becomes too hot, or gets dry.

To conclude, I would again say, that physicians ought to examine this water-cure system with care. Water is a remedy always at hand, and it is a powerful remedy. Why should we not try it, and use it if we find it valuable?

June, 1852.

ART. II.—*On the Treatment of Dysentery.* By ROBERT DURRETT, M. D., Resident Graduate at the Louisville Marine Hospital, during the year 1851.

There were admitted into the Louisville Marine Hospital, in 1851, forty-four cases of dysentery, of which number eighteen cases of an acute character were submitted to the saline treatment, in all of which except one it proved successful. The causes of failure in this case were undoubtedly the advanced stage of the disease and the extreme feebleness of the patient. The mode of treatment was as follows. The patient, according to his physical strength and habits, was ordered sulphate of magnesia, from one to four drachms every hour, till serous evacuations were produced, free both from blood and mucus. The patient was then ordered a Dover's powder, and generally an enema of water and morphia. It was not found necessary to repeat the sulphate of magnesia after the first day in any but a very few cases.

After the administration of the opiates, the patient usually fell asleep, and awoke declaiing himself relieved, and continued to improve under the administration of an occasional opiate. Enemas of spirits of turpentine ℥ss, mixed with an egg, repeated once or twice a day, or oftener if the severity of the symptoms demanded it, had the happiest effect in six cases. When there was much tenesmus, a drachm of tinct. opium was combined with the turpentine and egg. The patient frequently retained this injection without complaint for several hours, and almost invariably passed it off either with a good discharge from the bowels, or one was sure to follow immediately. In all cases of dysentery, where the inflammation does not extend beyond the rectum, I should be disposed to give the turpentine enema a trial. Many cases of chronic dysentery were treated satisfactorially with copper and

opium. Some cures were very happily effected with this article, in which time-honored remedies failed.

Louisville, June, 1852.

ART. III.—*A Monograph on Fractures:* Being an Inaugural Dissertation submitted to the Trustees and Medical Faculty of the University of Louisville, March, 1852. By JAMES KNAPP, M. D., of Louisville, Ky.

The subject selected involves the solution of continuity of that part of the human organism, the office of which is to give form and support to the other parts of the body, to protect the more delicately organized and vital parts, as the brain and organs of the thorax, from the effects of external violence, to give attachment to the muscles, and enable man to perform the various changes of position and place, which, constituted as he is, are so essential to the continuance of his being, as well as happiness. Notwithstanding that the bones are admirably adapted to enable him to perform the various movements of will, and often to avoid danger and resist violence, yet many are subject to a great variety of accidents, some of which bring about the change which is the topic for present consideration.

We find the bones of various forms, as long, broad, and irregular, and also differing in density and power of resisting violence; some more exposed to injury from the office they perform, or the position they occupy; it is not therefore strange that, from the varied external forces that may act upon them, the muscular contractions as well as many predisposing causes, fractures should so much differ, not only in form, in the modification of treatment they demand, but in the time which nature, with the artificial aid of the surgeon, requires to effect the reparation of the injury.

Fractures have been divided into two classes—*simple* and *complicated.* A *simple* fracture indicates a solution of continuity of the bone, the bone being separated into two parts only, and not connected with an important injury of the surrounding tissues.

A *complicated* fracture is one in which the bone is broken in more places than one, is comminuted or crushed, is connected with a dislocation, or where there is an external wound, with or without protrusion of a fractured extremity, or laceration of a large blood-vessel, nerve, or injury of the brain, thoracic or abdominal viscera.

The diagnosis in most cases of fracture is not difficult; there is generally more or less distortion and inability to use the limb, if it be an extremity fractured; shortening, usually, unless the fracture be transverse, or so situated that another bone acts as a splint in preventing it; there is also pain at the seat of fracture, aggravated by motion of the injured parts, also crepitus when one fractured extremity is rubbed against the other; and there will be swelling from the extravasation of blood, or effusion of serum into the surrounding integuments. The pain and swelling, and inability to use the limb are not peculiar to fractures, therefore the distortion and crepitus are the most important and the reliable diagnostic symptoms.

The prognosis depends upon many circumstances, as the constitution, age, and habits of the patient, also upon the particular bone injured; the long bones, excepting their epiphysis, uniting much sooner than the broad and more spongy bones, the inferior maxillary, clavicle, ribs, and bones of the fore-arm uniting in the shortest period, those of the upper extremities sooner than those of the lower, the broad and spongy bones, and the epiphysis of the long bones requiring the longest time, while the olecranon, and acromion processes, the neck of the femur internal to the capsular ligament, and the patella seldom unite by bone, but by a ligamentous substance; it is said

however, if the parts are kept perfectly coaptated, bony union may take place. Fractures in very old persons, and in some other bad conditions of constitution, sometimes will not unite at all; and in cases where there is too much motion allowed at the seat of fracture, bony union cannot take place, and the result will be the formation of a false joint.

The treatment of fractures consists, first, in relaxing the muscles that act in displacing the parts, and then, aided in most instances by firm yet gradual extension, and counter extension, placing their parts as nearly in their natural position as possible, after which the common roller, or in fractures of the lower extremities, the bandage of Scultetus should be applied, commencing at the extremity of the limb, sufficiently tight effectually to control the œdema and to confine the muscles, and prevent their contracting and displacing the parts. Splints, usually of wood, and as long as the fractured bone, and in some instances of greater length, having been padded with some soft material, as hair, tow or cotton, should be firmly bound to the limb, with pieces of tape or bandage, in order to prevent all motion at the seat of fracture, and retain the limb at its natural length and straightness. The dressings may require to be loosened when swelling comes on, and again to be tightened as it subsides, and if at any time an irregularity should occur, it should be immediately rectified. The parts should be kept constantly wet with cold water, or sugar of lead water, or in some instances warm water will be admissible; and quietude and the recumbent posture, in most cases, should be strictly enjoined; while the diet should be light and the medicines adapted to the symptoms of the case. In some instances, as where the patient is young and plethoric, and there is reason to believe there will be much swelling and high inflammatory action, blood should be abstracted; but in old and enfeebled constitutions, and where it is evident the vital

powers will be greatly taxed previous to recovery, blood-letting should generally be avoided. In complicated fractures, the various complications will require special treatment. If there be a dislocation, it should be reduced previous to the replacing of the fractured parts; if an artery be wounded, it should be tied, or if there be an external wound, it will require appropriate dressings, or if inflammatory action should be threatened in internal organs, it will require the usual treatment. But if the thigh bone, or bones of the leg, are very much broken, as in bad forms of comminuted fracture, and more especially, if near or complicated with injury of the knee joint, or if there be extensive laceration of the soft parts, together with injury of a principal blood vessel, or nerve, it may be advisable to amputate as soon as the shock of the injury has been sufficiently recovered from, or if after any fracture of an extremity, traumatic gangrene should commence, amputation should at once be performed.

The question may now arise, how are broken bones reunited? It is evident when a fracture takes place, that not only is the osseous matter divided, but also the vessels and nerves of the bone, the medullary substance, and periosteum; and also that the surrounding textures are more or less injured; blood is effused; the reparatory process soon commences by a deposit of coagulating lymph, which stage is followed by that of fibro-cartilage, then cartilage, and finally that of ossific matter. Meanwhile, blood-vessels and nerves have become extended through the new formation, and the medullary cavity becomes re-established, the new bone having passed through precisely the same stages of formation as those of original bone.

Having thus far considered fractures in general, particular fractures will now claim attention; but in treating of the latter, my intention is to be as brief as possible, and not to dwell upon much but what is peculiar to each.

The cranium from its occupying the highest part of the osseous system, and from its being a wall of defense to a vital organ, the most wonderful of all organs, the organ of mind, serves as a very suitable point from which to commence this part of the subject.

Fracture of the *cranium* may occur where the direct force is applied, or at a distant part, may consist of a simple fissure without depression, or both tables may be depressed, or the external table in middle-aged persons may be driven into the diploë, while the internal table remains uninjured, or the internal may be fractured and depressed, and produce upon the brain all the effects of compression, while the external remains entire. I need only add, that if there be depression sufficient to produce symptoms of compression, or if it be a punctured fracture, the depressed parts should be restored, or detached spicula removed by the aid, in most cases, of the trephine.

Fracture of the *ossa nasi* will be recognized by the depression and irregularity, which in most instances, is quite manifest.

The reduction may be effected by passing a strong probe into the nostril, then with the fingers placed externally, the parts can easily be coaptated. The parts will retain their position without dressings.

Fracture of the *inferior maxillary* may occur at any portion of its extent; its most usual situation is near the middle of the horizontal ramus. The fracture will be known by the irregularity of the teeth, and laceration and bleeding of the gums. If there are loosened teeth they should be replaced; and the fractured parts brought evenly together, and if there be much tendency to redisplacement of the fracture, a tooth that is firm in its socket on one side of the fracture may be fastened to one on the other side, with a silver wire or silk ligature; the teeth of the lower jaw, should then be placed firmly against those of the upper, a compress reaching from the angle of the

jaw to near the chin, should be placed beneath the jaw, a roller should then be passed by several successive turns under the jaw, up along the side of the face over the head, then several times horizontally, encircling the temple, occiput, and forehead, then by additional horizontal turns carried around the back of the neck, under the ear, over the point of the chin; a piece of roller may then be fastened to the turn at the middle of the forehead, and carried back over the top of the head, and secured to the one at the back of the neck; and pins should be inserted at each point where the roller has crossed.

Fracture of the *clavicle* is usually produced by a fall upon the elbow or shoulder. The shoulder will be depressed and fallen forward and inward, the distance between the acromion and sternum will be shortened, and there will be irregularity at the seat of fracture. The shoulder should be raised and carried outward and backward; a wedge-shaped pad should be placed in the axilla, with its large extremity upward, and confined by turns of the roller passed around the body including the pad; the elbow having been carried forward upon the chest, should be firmly supported by the roller, passed by several successive turns from the opposite shoulder over the fore-arm near the elbow, then under the elbow, around behind the chest again to the shoulder; after which the roller should be passed horizontally around the body including the arm, and the fore-arm should be placed in a sling.

Fractures of the *scapula* are of rare occurrence, yet they may take place at its neck, acromion, and coracoid processes and inferior angle. The symptoms of fracture of the neck, are depression under the acromion, and crepitus when the shoulder is moved; the part can easily be replaced, but will immediately become again displaced when the support is withdrawn. The reduction is effected by elevating the head of the humerus and pushing it outward; the position should be retained by a wedge-shaped

pad in the axilla, as before directed, and the roller so applied as to keep the elbow firmly elevated, and the fore-arm should be supported.

The fracture of the *acromion* may be known by the depression at the point of the shoulder, the fractured portion being drawn downward; by the irregularity felt by tracing the spine of the scapula and by the crepitus that may be produced when the fragment is restored, and the arm rotated. The arm should be raised in order to restore the fragment, a wedge-shaped pad with its large extremity downward, should be placed between the body and the lower portion of the humerus, and the elbow should be kept well supported in order to keep the fragment sufficiently elevated, and the arm should be confined to the chest, and the fore-arm placed in a sling.

The fracture of the *coracoid* may be known by the inability to carry the arm forward and upwards, also by pressing with the finger upon the fragment. The arm should be brought forward and confined to the chest, and the fore-arm supported in the usual way.

The fracture of the *inferior angle* may be detected by fixing the point and then moving the shoulder. The arm should be carried forward upon the chest, and a compress placed against the fragment; a roller should then be carried several times around the body confining the compress against the angle, the arm should then be included by a few turns, and the fore-arm should be kept in a sling.

Fractures of the *humerus* have been divided into those of the neck, shaft, and inferior extremity.

The fracture of the neck may be recognized by the inability to raise the arm, by the irregularity that may be felt by pressing upon the fracture from the axilla, and by observing that the head rests in the glenoid cavity, and does not move with the shaft when the arm is rotated. The parts having been coaptated, and a roller applied to the entire extent of the limb, a splint should be applied,

extending from the olecranon to the axilla, one from the bend of the arm to the acromion, another from the external condyle as high as the second; these should be secured to the limb by turns of the roller; a wedge-shaped pad should be placed in the axilla, with its large extremity upward if the lower fragment is drawn inward, but the reverse if it is drawn outward; the arm should be confined to the chest, and the fore arm rest in a sling.

The fracture of the shaft will require a roller, four splints, and a sling; care must be taken in this, and in fracture of the neck, that the arm is not elevated by the sling.

The fracture of the lower extremity, like other fractures near a joint, should receive much care in diagnosis. The existence of the fracture having been ascertained, the parts should be coaptated, and the limb bandaged and retained in a flexed position; two pieces of paste-board each cut at right angles, should be applied to the limb, one to the inside, the other to the outside, over which the roller should be applied, or in order to afford more firm support, angular wooden splints may be used. The angular splint with a screw will be found most convenient for varying the angle of the limb; daily passive movement of the joint should not be neglected in this or other joints near a fracture as soon as the parts have so far united as to admit of it; by this means anchylosis may frequently be prevented.

Fractures of the *fore-arm* are divided into those of the olecranon and coronoid processes of the ulna, of the shafts of the radius and ulna, and of the lower extremity of the radius.

The fracture of the olecranon may be the result of muscular contraction or external force. It may be known by the inability to straighten the arm while it can readily be flexed; by the inability of the fragment, and by the depression at the back part of the elbow, caused by the ole-

cranon being drawn up the arm, sometimes to the distance of one or two inches. The arm should be placed in a straight positton, the fragment can then be restored; a compress should be placed against the upper portion of the fragment; a roller is to be applied from the lower extremity of the limb, and passed in the form of the figure 8 around the elbow, confining the compress against the fragment, then continued to the shoulder, in order to prevent contraction of the triceps muscle, and a straight splint suitably padded, should be confined to the fore-part of the limb.

The fracture of the *coronoid* is rarely met with, yet may occur from contraction of the brachialis muscle. There will be difficulty of bending the elbow, and the ulna will be dislocated backward. The ulna should be drawn to its place, the limb bandaged, and an angular splint applied. The angular splint should also be used in other cases of fracture of the fore-arm near the elbow. The fracture of the shafts together, or singly, may be known by the irregularity; and crepitus when the humeral extremity of the fore-arm is fixed, and the other extremity rotated. The fracture may be reduced by making pressure along the interosseous space while extension is made at the wrist, and counter-extension at the elbow; then two splints of at least three inches in width, to extend from the elbow to the points of the fingers, and padded most thickly along their middle line, in order to press the muscles into the interosseous space, should be applied, one to the palmar, the other to the dorsal surface, and confined by the roller; or the compress may be first applied, then the roller, and then the splints. The fore-arm and hand should be supported in a sling with the thumb uppermost.

The fracture of the lower extremity of the *radius* should be treated as if situated near its middle, with the addition of a compress along the dorsal, another along the palmar side, over the most projecting parts.

Fractures of the *carpal and metacarpal* bones will require a soft compress to be placed in the hollow of the hand, and a padded splint to be applied upon the palmar, another upon the dorsal side, extending from the elbow to the points of the fingers.

Fractures of the *phalanges* should be dressed with a roller, and four splints, two of which should extend from the wrist to the point of the finger.

Fractures of the *ribs* may be known by the lancinating pain when the parts are moved; by the irregularity at the seat of the fracture; and by the crepitus when the hand is placed upon the part and the patient coughs. A wide roller should be passed several times, tightly around the chest, and shoulder pieces should be attached to the turns to prevent their slipping downward.

Fracture of the *sternum* should be treated in the same way as fractured ribs.

Fracture of a *vertebra* may occur either at the body or spinous process. If the body be fractured, no attempt should be made to reduce the fracture, for such effort would probably be attended with the immediate death of the patient. Thorough antiphlogistic treatment should be early adopted; although recovery cannot be anticipated.

When the *spinous process* is fractured it should be replaced, and retained by a compress and roller; this fracture, unless complicated with injury of the spinal cord, will result favorably.

Fracture of the *os innominatum* can be produced only by great force; and is usually attended with some fatal complication. If there be displacement of the fragments the muscles acting upon them should be relaxed, and the parts replaced; a broad bandage should then be placed around the hips, and the remaining treatment adapted to the circumstances of the case.

Fracture of the *os coccygis*, or lower portion of the *sa-*

crum, may be reduced by passihg the finger into the rectum and pressing upon the displaced part; the bowels should be kept relaxed.

Fracture of the *femur* may take place at the neck, shaft, or inferior extremity. The fracture of the neck may be within, or external to the capsular ligament; the limb will be shortened and generally everted; and if it be drawn to its former length and rotated, crepitus will be manifest; if this extension be discontinued the limb will again become shortened and everted. A foot-board with two mortices should be provided; also two splints, each long enough to extend from the axilla to several inches below the foot; the upper extremities should be prepared like a crutch, the lower to pass through mortices of the foot board; also two long bags filled with chaff to apply to the insides of the splints, and a third to be placed between the limbs; a mattress, tapes, and a splint cloth should be arranged, and the patient placed upon them; the fracture should then be reduced, the feet confined to the foot-board, the long bags applied to the splints; and rolled in the splint cloth, the lower ends of the splints passed through the mortices and firmly secured, the upper ends bound to the shoulders; the third bag placed between the limbs, and lastly the tapes should be tied.

Fracture of the shaft has been successfully treated according to the following plan: A mattress should be provided, upon which a sheet should be placed, then pieces of tape at intervals of ten inches, over these a splint cloth a yard and a half long, and as wide as the length of the limb, over this a strong piece of paste-board to apply to the posterior part of the thigh, and to extend several inches above and below the fracture, then strips of scultetus bandage; and a large handkerchief should be placed at the upper edge of the splint cloth, another at its lower edge. Splints should also be prepared corresponding in width to the limb, one to extend from the axilla to six

inches below the foot, one from the perineum to the same distance below, another to be of the length of the femur; the long splint should be formed and padded at its upper extremity like a crutch, a short distance below it should have two mortices, also one near the lower extremity, a little above a small block of wood should be attached, then two bags of suitable width and as long as the limb, should be prepared, and filled with chaff. The patient should then be placed upon the bed, and the limb placed upon the strips of bandage; one handkerchief should be used to produce extension at the ankle, the other counter extension at the groin, meanwhile the broken extremities should be placed in apposition, the bandage applied, the splints rolled in the splint cloth, the bags interposed between the splints and the limb, the extending and counter extending bands passed through the mortices and tied; then the anterior splint should be applied, the tapes tied, and a bandage passed around the pelvis, including the long splint. Care should be taken that the limb is drawn to its exact natural length, and the parts placed in their former relative position.

The fracture of the lower extremity may be treated upon the same plan as the fracture of the shaft.

Fracture of the *patella* is usually transverse, and generally caused by muscular contraction. The limb should be placed upon a padded straight splint, and kept somewhat elevated; and during the first few days means should be adopted to subdue the inflammatory action; this having been accomplished, a bandage should be placed above, another below the patella, and these should be approximated on each side of the patella by strips of tape; care being taken that the fragments are kept in close contact. The other forms of fracture of the patella may be kept approximated by the aid of the straight splint, compresses, and a roller.

Fracture of the *leg* may occur in both bones together,

or in either singly. If the tibia is broken near its upper extremity the fracture should be reduced, and a scultetus bandage applied; then two splints should be prepared to extend from the upper part of the thigh to several inches below the foot, and the outside splint should be provided with a foot-piece; they should then be well padded, and confined to the sides of the limb; and the limb should be kept somewhat elevated. Fracture of both bones at their middle or lower portions, should be treated with the scultetus bandages, and two splints to extend from the knee to below the foot, and foot-piece. If only one bone is broken, but one splint need be used, and that should be placed on the side opposite the fracture. Care should be taken to keep the foot from turning too much inward or outward, and from falling below a proper line with the leg.

Fractures of the *foot* are frequently attended with severe injury of the soft parts, making amputation necessary. The fracture of the os calcis will require a bandage to be applied above the knee, and the knee to be kept flexed, and the heel elevated by a piece of tape drawn tightly from the heel of the slipper to the bandage. The other fracture of the foot will require the application of a roller, compresses, and splints so used as to preserve the proper position of the foot.

Art. IV.—*Chemical Examination of the Mineral Water of Cooper's Well, near Raymond, Mississippi.* By J. Lawrence Smith, M. D., Professor of Chemistry in the University of Louisiana.

The water which forms the subject of this report, is one which has enjoyed a high reputation for medical virtues during several years, it being more especially beneficial in chronic diseases of the intestines.

The water analyzed, was collected in the month of December, 1851, and part of the examination was made at the source.

To most persons who visit this watering-place, its discovery is associated with some curious dreams of the remarkable individual whose name it bears, but its medical virtues remained unknown for sometime after its discovery, and were first ascertained by persons living in its neighborhood. The water is derived from a well dug to the depth of 107 feet in a porous sand rock, which is covered in some places with a coarse conglomerate of pebbles, consolidated by oxide of iron. The depth of the water in the well seldom exceeds five feet; it is said to flow in at the bottom from three different sources, and it will be well to examine their general characters separately, so soon as the water from them can be obtained. It flows most abundantly in winter, and therefore the summer water might be expected to contain more mineral matter. The country around is broken, remarkably dry, enjoying a reputation for general salubrity, and its convenient proximity to the Vicksburg and Jackson railroad is calculated to make it the first watering-place in the South-Western country, either as a resort for invalids or pleasure seekers.

Chemical Examination of Cooper's Well Water.—Temperature, 64° Fahr., the air being at 53° Fahr.

Taste—Not unpleasant; slightly mineral.

Odor—Little or no odor, although at times, it is said to have distinctly that of sulphuretted hydrogen.

Color—Transparent, with small yellow flakes floating in it.

Specific gravity, ... 1,00,147.

Gas, contained in one gallon,

Composed of oxygen, ... 20

" " nitrogen, ... 68

" " carbonic acid,—trace;

" " sulphurretted hydrogen,—trace.

Substances contained in one gallon, are,

Sulphate of soda,	11.705	grains.
" " magnesia,	23.280	"
" " lime,	42.122	"
" " potash,	.608	"
" " alumina,	6.100	"
Chloride of sodium,	4.260	"
" " calcium,	4.025	"
" " magnesium,	3.480	"
Silica,	1 801	"
Peroxide of iron,	3.362	"
Crenate of lime,	.311	"

The deposit which collects from concentrating the water, or after it has stood for a time, contains in 100 grains:

Crenate of lime,	2.
Peroxide of iron,	35.
Sulphate of lime,	25.
Water,	38.

The iron was found altogether in the yellow particles which float about the water, although it is more than probable that at certain seasons of the year it must be found in the clear water also.

The water when kept loses none of its properties, as has been proved by strict analysis; and at all times, when the effects of the iron are sought for, the sediments should be drank along with the clear water.

The concentrated water appears to lose nothing but a portion of its sulphate of lime. So in that form, when diluted with pure water, similar medical effects might be expected to those produced at the spring; but of course like all other mineral waters, it is only at the source that the patient can expect most advantage; and it is equally true that precautions are to be observed in the use of Cooper's well water.

The diseases to which the water appears mostly adapted, are chronic intestinal diseases, which embrace a class

too numerous to be mentioned in this report. It has also been found eminently useful in dropsical affections, as well as in inflammation of the bladder. In the short space of twelve or fifteen years, it is impossible to discover all the diseases to which a water of this description is adapted, as chemical analysis alone cannot guide us in making its application. The remarkable virtues of this water, in the character of diseases before alluded to, are known and acknowledged by several of the most eminent physicians of New Orleans, Mississippi and Alabama.

BIBLIOGRAPHICAL NOTICES.

ART. V.—*Tableaux of New Orleans.* By BENNET DOWLER, M. D.

This pamplet gives a great deal of valuable information, geographical, commercial, geological, and sanitary, respecting one of the most important cities in our country. It is written with all the learning and laborious industry for which its author is distinguished. Under the head of "*The Necropolis of New Orleans,*" he has the following:

In the year 1540 I ascertained, by careful inspection, the ages of a great number of persons buried in the cemeteries of New Orleans, copied from monumental inscriptions, taken at random, that is, without picking or selection, in series of thirty each. The numerical results of this inquiry, in the absence of more authentic registries of births, deaths and ages, may be valuable in determining approximatively the duration of life, and the sanitary history of the climate of New Orleans. The necrological inscriptions in the different cemeteries of this city are not only illustrative of its vital history and sanitary perturbations, but are also, to a considerable extent, characteristic and peculiar.

The ideal of vital statistics, as a method, presents sundry conceptions as absolutely true, in advance of empirical processes, and their actual results in relation to both the statics and dynamics of population. But how clear soever the theoretical principle may be, its useful application is alone to be found in the experimental.

For scientific purposes, the facts of vital research and reasoning must be numerous, continuous, and must extend through long cycles of time. The most accurate record, comprehending the history of every individual in

existence for one year, or one dozen of years, would utterly fail to solve some of the most important vital problems, which anterior to experience and its results, are known to be solvable, and within the legitimate range of empirical possibility. Synthetical reasoning determines, in advance of experience, that the mean duration of life in a country can be inferred from a record of the ages of all the natives dying within its limits. The record of one entire generation would not only show the exact duration of the mean life for that period, but in connection with a sufficient number of similar previous records, would indicate, with slight and unimportant variations, the mean duration and expectation of life for succeeding generations. In the science of population, however, a generation commencing at a given era, would not be complete as a vital cycle until the last individual then living shall die. For example, an individual born in 1851, might not die until 2020, provided he should live as long as Henry Jenkins did—that is, 169 years. A registry of an entire generation will have been completed at the death of the oldest individual. The sum of all the ages of a given generation, divided by the whole number of individuals, will give the mean age.

Without any regard to the actual census, suppose the fixed creole and acclimated population of New Orleans to be 100,000, and that immigration and emigration be wholly arrested;—suppose four children to be born and four persons to die annnually for every one hundred inhabitants; then the population will be stationary, without increase or decrease, and how much soever individual ages may differ, the average life will be twenty-five years. But in case only two die in the hundred, the births undiminished, the population will increase annually two in the hundred, or for the city two thousand; the whole number of births will be four thousand, and the whole number of deaths two thousand; then the average life will be double—will be fifty instead of twenty five years. Suppose that during the year 100,000 persons come temporarily into the city, and lose two in every hundred, all the survivors returning home, then the deaths and births will be equal, the average life will seem to be reduced to twenty-five, and the increase neutralised; all of these conclusions would be necessarily false, for want of correct data, com-

prehending all the essential elements entering into the numerical process, static and dynamic.

Although four births and four deaths annually for every one hundred persons would cause the population to be stationary, and the mean life to be twenty-five, yet, with respect to the latter, a single year might give a result altogether different; and so of a very limited number of years; thus, one dying aged one hundred, another fifty, a third twenty, and a fourth ten, the average might be double that which has been named. But in a period sufficiently prolonged, such excesses would be reduced or equalized. In tossing a dollar in the air the same side might fall uppermost for several consecutive times; but if the operation be continued sufficiently long, this marked inequality disappears forever. The same principle prevails in vital statistics, as well as in numerical medicine.

There are, indeed, a great many elementary questions which it is desirable or necessary to consider, in order to arrive at satisfactory conclusians concerning the vital condition of New Orleans.

What is the annual mean number of the resident creole and acclimated population? What is the annual mean number of strangers, and time of their temporary residence? What is the mean number of births, deaths and ages of each class, not to mention internal and external relations, topograthical, ethnographical, physioiogical, sanitary, industrial, pecuniary, educational, moral, religious, social, civil, in the white, black, red, and mixed races?

By the general custom of mankind—one not only in accordance with good taste, but with sanitary requirements —the dead are consigned to the ground—"earth to earih;" but in New Orleans a different method of sepulture prevails. In most of the cemeteries, interment in the ground is wholly interdicted, elevated vaults and tombs only being used. The necessity of this method of entombment, for all who can afford the expense, is easily explained by referring to the topography of the city. A grave in any of the cemeteries is lowor than the adjacent swamps, and from ten to fifteen feet lower than the level of the river, so that it fills speedily with water, requiring to be bailed out before it is fit to receive the coffin, while during heavy rains it is subject to complete inundation. The great Bayou Cemetery is sometimes so completely inundated

that inhumation becomes impossible until after the subsidence of the water; the dead bodies accumulating in the meanwhile. I have watched the bailing out of the grave, the floating of the coffin, and have heard the friends of the deceased deplore this mode of interment. A young Irish woman, on seeing her husband's coffin lowered into a grave of welling water, exclaimed repeatedly; "Oh, Mike, it is a dear burying to you to be buried at the Bayou! Oh that you should come to this!" It is this feeling that has built the different cemeteries which constitute the great necropolis of New Orleans. Interest, to say nothing of the vanity of friends, requires inscriptions to identify a vault, which is private property, purchased under a written title or conveyance. Hence, these monumental inscriptions, from their constancy, accuracy and number, afford data which, in the absence of exact registries, are probably more trustworthy and valuable than can be found in any other existing necropolis. These necrological monuments will augment from generation to generation, and must hereafter prove more useful to the vital historian than the pyramids of Egypt, or the countless millions so carefully embalmed and deposited in the catacombs of that country, forty centuries ago. The ethnologist might even now commence his lesson among the tombs. The Caucasian is separated from the negro race. In some cemeteries the Irish, in some the German, in some the Anglo-American, in some the French type predominates.

The monumental evidence to be offered in this tableau, in relation to the salubrity of the city and the length of life, compared with other places, is doubtlessly imperfect. The principal objection to which it is liable, appears to be this; namely, very young children may not have had inscriptions on their vaults, as constantly as adults; though this hypothesis may be incorrect. But admitting that it is true, this source of error is neutralized, it may be supposed, by an undeniable fact, that in all the cemeteries, even those which reflect the creole life most truly, as the Catholic, strangers, victims to the climate, who "liveth not half their days," are buried, and being counted, tend to shorten the average life probably as much as the supposed omission of infantile inscriptions tends to enhance it. The evidence, upon the whole, if not demonstrative

possesses probability, and is offered for what it is worth, in the absence of more exact data.

In the following enumerations, fractional parts of a year are reckoned as one year when they exceed six months, or fall short of eighteen months, and so of all fractions in more advanced ages. In all cases it was deemed necessary, in recording a series of ages, not to reject any because they were short, nor to seek any because they were long. Thus, on one occasion, having completed the series for the time and the place, I came immediately to an inscription upon a well known negress, aged 107 years and 5 months, born in 1732, died in 1839, but the rule adopted excluded this, as well as other similar cases. In Lafayette cemetery, as the sexton informed me, there is a negress slave buried aged 110. A similar age was found in the Catholic cemetery, after having finished the series. But all these were omitted.

The old Catholic cemetery, (No. 1, Basin street,) in which nearly all the inscriptions are French, 13 only were distributed among all other languages, gave the following results, after having made 136 observations:

The first series of		30	observations	gave	an	aggregate	of	1474	years.
The second "	"	30	"	"	"	"	"	1512	"
The third "	"	30	"	"	"	"	"	1381	"
The fourth "	"	30	"	"	"	"	"	1313	"
The fifth "	"	16	"	"	"	"	"	852	"
Total observations, 136.								Total ages 6537:	

mean life 48 years and a fraction: more than 21 years over the mean of the Hebrew cemetery—20¼ over that of the Bayou; 17¼ over that of the Protestant;—27⅓ over that of Lafayette city;—12 over that of all France;—nearly 20 over that of the department of the Seine, (Paris)—and about 22 years beyond the mean of the old Protestant cemetery immediately adjacent. The following table shows the mean age, with the three oldest persons in each series, in this cemetery:

Series.	*Mean Age.*	*Three oldest in each Series.*			*Mean Age of Three Oldest.*
1st. series	49.01	81	80	76	79
2nd. "	50.56	76	76	74	75.33
3rd. "	46.03	85	80	78	81
4th. "	43.76	85	81	72	79.33
5th. "	53.25	92	90	90	90 66

Although the place of nativity is not always mentioned in these inscriptions, yet out of Louisiana the United States furnished but 1, and Ireland but 1, France 19, and Spain, Genoa and St. Domingo, each 4. The prevailing type, in this cemetery, is doubtlessly the creole French.

The old Protestant cemetery, (adjoining the Catholic cemetery on Basin street) long abandoned as a place of burial, gave for 30 inscriptions an aggregate of 797 years, and a mean life of nearly 26½ years—the three oldest, 62, 60, 47.

The new, and by far the most extensive of the Catholic cemeteries, is that in the rear of the former, consisting of four squares, between Robertson and Claiborne streets, the southern portion of which is for the colored race. In this cemetery, especially in its northern portion, French inscriptions preponderate. The white race, in 80 observations, afforded the following results: The first 30 gave an aggregate of 1296, and a mean of 42.2 years—the 3 oldest 89, 77, and 74; the second 30 gave a total of 1415; a mean of 47.16; the 3 oldest 80, 75, 72; the residue 20 observations gave a total of 997; a mean of 49.85 years; the 3 oldest 83, 80, and 75.

The aggregate of these 80 observations amount to 3678 years, giving a mean age of nearly 46. (After counting these 80, one was found aged 110, though I could not count it consistently with my plan, which rejected the principle of selection.) In the middle division of this cemetery, 30 inscriptions gave an average life of nearly 47½ years.

By uniting these divisions of the Catholic cemetery No. 2 with that on Basin street, the observations will amount to 396—the aggregate 18 607 years, and the mean life of the whole, both of the whites and blacks, will be very nearly 47 years.

Of these 396 inscriptions, 49 were over 70;—13 were over 80;—5 over 90.

The black race in this cemetery, buried in a style of magnificence nearly equal to the white, has usually French inscriptions, indicating as the principal places of nativity, Louisiana, St. Domingo, Cuba, Jamaica, and Africa, and gave, in 150 observations, the results which the following table expresses with the utmost brevity: There may be some error in the third series—a discrepancy there seems

to be, inasmuch as this series gives a comparatively diminished total and mean life.

Series Thirty Obs. Each.	*Aggregate Ages Of Each Series.*	*Mean Ages of Each Series.*	*Three Oldest In Each Series.*		
1st. Series,	1594	53.13	100	85	80
2nd. "	1364	45.46	64	80	75
3rd. "	1102	37.4	95	82	70
4th. "	1318	43.93	100	83	79
5th. "	1585	52.0	100	92	90
Total ages of 150,	6,969				
Mean age of 150 persons, - -	43.43 years.				

The united ages of the fifteen oldest persons in this enumeration amount to 1,398 years, affording an average life far beyond "threescore and ten," (the limit indicated by the royal poet of the Hebrews,) namely, 86½ years, with two centenarians for every hundred; or as many of that age as France affords in about a half million. Probably the entire number of vaults and tombs in the African cemetery does not exceed two thousand, nor the dead bodies exceed three thousand. Now, on the supposition that by some strange and incredible chance, the one hundred and fifty inscriptions I took note of, actually exhausted the whole number of centenarians, (which I know was not true,) still the colored centenarians, transcend French centenarians, two hundred and fifty times.

It will be seen that the black race affords, by these tables, 1 in 50, aged 100 years; and if we add 11 years to the lives of the remaining two oldest in the 150 enumerated, the result will be, five centenarians; or 1 in 30; or 8,333 times more than the ratio for all France; or 2,100 more than that of England, by the census of 1841; or if we take the official account of the deaths in France for the 15 years ending on the first of January, 1852, it will be found that 150 inscriptions give for the black race in New Orleans, nearly one fifth as many centenarians as 11,793,-289, or nearly twelve millions, of deaths among the French. But, by an exact calculation, the French bills of mortality, as above mentioned, give one aged one hundred in every 471,731; the black one in fifty.

Each of the remaining cemeteries of New Orleans, as they contain a greater proportion of strangers, will be

found to offer a rapid decrement in the mean life. The new and extensive Protestant cemetery of the Second Municipality gave, in the first 30 observations, as the three oldest, 73, 43, 40; the second 30 gave, for the three oldest, 78, 69, 66. From 110 observations, a mean life was obtained of 30¾ years. The Hebrew cemetery gave, as the three oldest, 74, 63, 62, and an average of 27 years.

The Bayou cemetery, or Potter's Field, not having monumental inscriptions, with few exceptions, proved an unsuitable field for necrological researches. From the rude and frail memorials of the dead, I obtained thirty-five ages; the oldest three were, 55, 52, 45—the mean life of the whole, 27¾ years—a mean nearly twenty years less than that of the old Catholic, and the African cemeteries.

The city of Lafayette, separated from New Orleans by a street only, abounds with German immigrants, who, with the Irish, are in both cities the principal victims of yellow fever. The Lafayette cemetery is more favorable for inhumation in the ground than the New Orleans cemeteries; accordingly, this mode of sepulture is more common in the former. Among 30 ages taken from the vaults of that cemetery, 39 was the oldest, and the mean of the whole was only 20¾ years, which is the minimum of all the cemeteries, being 26 years less than that of the black race in the Catholic cemetery, and nearly two and a half times less than that on Basin street.

The Catholic cemeteries are supposed to reflect the creole life more accurately than the other cemeteries, which are newer, and have been filled with immigrants. The mean life, as deduced from monumental evidence, though not identical with that deduced from the recent mortality of the city, by the Board, is confirmed by the latter; that is to say, the Catholic cemeteries take precedence of the Protestant, and the Protestant of the Potter's Field. Any one acquainted with the different classes of the population would have anticipated these results.

The following is interesting, on the "*Sanitary History of New Orleans:*"

There is not, probably, any considerable city in North or in South America, including the West Indies, whose sanitary history is so favorable as that of New Orleans,

until near the close of the last century. Not long after the foundation of the city, the Parisian press teemed with works on Louisiana, its climate, topography, botany, zoology, and its native Indians. Some of these works were of an official character, having originated in scientific expeditions ordered by the French Government. Charlevoix's work, [3 vols. 4 to.] describes the city 4 years after its foundation in 1718,—animadverts upon its topography, estimates its population in January, 1722, at 200, who lived in huts upon the river's bank, the regular houses not having been as yet constructed. Laval's work on Louisiana was published in 1728. In neither of these works is the salubrity of the climate questioned. Dr. Pratz, in his history, [3 vols. 1758,] maintains that life in Louisiana was not only pleasant but long. La Harpe, the agent of the French Government, sent to explore the boundaries, climate, &c., of Louisiana, arrived at New Orleans in the fall of 1718. In 1724, after having spent five years in Louisiana, chiefly in New Orleans, he returned to France. His journal is minute, but contains no account of fevers, or other epidemics in the colony. He estimates the inhabitants of the city, including the troops, at sixteen hundred—the climate as being temperate—the air as salubrious; [355, 356.] The people, he continues, were entirely exempt from the epidemics which desolated other parts of North America. New comers, for the most part, were attacked with a slight fever [*une fivre lente*] which caused debility, but never death; [on ne voit pas de personnes en mourir.] The province, which had a black population of 1,000, was everywhere salubrious, particularly along the sea shore.

Lozieres, in his second voyage to Louisiana, 1794 to 1798, [2 vols., Paris, 1803,] asserts the salubrity of New Orleans, and seems much puzzled to explain it. He concludes, however, that the cause must be sought in the universal use of the Mississippi water; [T. i., 313.]

In a work published in Paris in 1802, edited by Duvallon, founded on a three year's actual residence, Louisiana is described as exempt from epidemic diseases, its fevers were mild, and rarely dangerous; the rate of mortality was small; whites and blacks of both sexes lived long, and were still fresh and vigorous at the age of sixty. The writer gives a flattering account of the physical conform-

ation, fine complexion, and rare beauty of the *dames* creoles. The sanitary condition of the city he maintains as being good, with the exception of the yellow fever. He says that this malady was not proper to the place, and had not been known until within the six or seven years preceding 1802, a period during which it had prevailed nearly every summer, and was attributable to the commercial intercourse with the North Americans, and to the increased heat of the climate incidental to the clearing away the forest, and the exposure of the soil to the sun's rays.

This author asserts that from the end of October to the beginning of July, there is in both town and country but little sickness, and that death but rarely occurs. During the hot season of three years preceding 1803, the mercury arose no higher than from 24° to 26°.; [Reau,] nor did it descend in the winters lower than two degrees below the freezing point.

Robin, in his travels in Louisiana from 1802 to 1806, [3 vols., Paris, 1817,] avers that the country has but few chronic diseases, diarrhea being the most common and fatal. He mentions sore throat as sometimes prevailing among children; also, infantile tetanus, [le malde de machoire,] together with yellow fever, which a few years previously had appeared for the first time in the city, and which he identifies by Father Labat's description, enlarging on its pathology as a disease of the blood, or rather as a rarefaction of that fluid, causing dilatation, tension and compression in persons from the North, whose blood he supposes is too rich and dense. Tropical acclimation, according to him, renders the blood thinner, lighter, and less dilatable.

The celebrated statesman, Count de Vergennes, in a memoir on Louisiana, addressed to his Government during the early days of the American Revolution, says: "I repeat what I have so often said, that Louisiana is, without question, the first country in the world as to mildness of climate, and its happy situation."

The immigrant to New Orleans runs a risk of the yellow fever during the first few, say 3 to 4 years, a great proportion escaping altogether, and a few suffering an attack after a more prolonged residence,—one attack with continuous residence protecting against subsequent at-

tacks. His dangers from epidemic fevers are, therefore, compressed within a few years, while in Northern latitudes, or even beyond the limits of the city, he is, during life, exposed to the risks of epidemic, bilious, remittent, intermittent, congestive, typhus, and typhoid fevers.

The yellow fever is emphatically the strangers' fever; individuals long resident, and natives of the city, being as little liable to take this disease as the vaccinated are to contract small pox.

Independent of emigrants, New Orleans receives within its limits for a few days or months a greater proportion of floating population than any other city, arising from its commercial character, geographical position, easy access by rivers, lakes, and seas, and from its public charity, thus becoming the asylum of foreign paupers passing through or temporarily resident in it. To these may be added sick and destitute boatmen, seamen, ditchers, wood-choppers and raftmen from the valley and swamps of the lower Mississippi, not to name a vast many poor consumptives seeking the benefit of the climate. Besides, New Orleans has been, and continues to be, the recipient of the broken down soldiers, the *debris* of the armies of Generals Taylor and Scott. The unparalleled California emigration contributes in the same way.

It is a problem unsolved by experience, whether a migration of the natives of these hot countries to the icy circle, would not be more fatal than that of the Northerners towards the torrid zone. Would not northern typhus, with pulmonary diseases, transcend in mortality southern remittent and yellow fever?

But in no case can acclimating diseases be received as the precise standard of salubrity or insalubrity for the entire, much less native population.

The sanitary statistics of New Orleans have been derived, in too many instances, from the books of the Charity Hospital, which is virtually a great foreign institution, located in, but not for New Orleans. Thus, in twenty years, ending in 1850, the admissions of patients into that institution [according to Dr. Fenner's valuable Reports] amounted to 123,917, of which number only 1,293 were Louisianians, that is to say, about one in every hundred. But probably not more than half of these were citizens of New Orleans; that is, one in every two hundred. Admit-

ting, what is generally conceded, that the hospital admissions represent from one-fourth to one-third of the whole number of sick strangers; but taking the latter ratio as a guide, it follows that 371,751 strangers are to be charged to the sick list of the actual citizens of the town.

The report of the Board of Health of New Orleans, for 1849, [one the least flattering to its sanitary character ever made,] shows that in the Catholic cemetery, for four years, ending in 1844, among 442 deaths there were ten aged more than one hundred years! Now, it requires nearly two and a half millions of people in France to produce ten centenarians,* according to the census, and still more according to the official bills of mortality. The entire department of the Seine [Paris] would have only eight centenarians, provided it had *ten millions of inhabitants*,* which is nearly one-third of the population of the entire Republic. New Orleans, according to this ratio, gives, in its necrological record, one in forty-four aged beyond one hundred years; while, by the census of 1840, there is in the United States but one in 6,157 aged one hundred years. In England and Wales there is but one of this age in every 55,555; in France, one in every 250,000. The centenarians in the Catholic cemetery are, therefore, nearly six thousand times greater [in equal numbers] than in France. Even the Potter's Field, according to the Report, in 8,566 gives nine centenarians, or one in every 951, that is, about 26.3 times more than the French Republic.

The Report asserts that "the average age at death in the Northern cities, [doubtless owing in a great measure to the large mortality in infantile life,] is from nineteen years, nine months, to twenty years, three months, and in some of the cemeteries, where destitute foreigners from the crowded parts of the city of Boston are buried, it is reduced to 13.43. * * * * In the city of New Orleans, the average age at death for the last year was 26.69, and for a series of years, the aggregate of all the cemeteries was 22.63." Is an increased average life to be considered a proof of the insalubrity of our climate, espe-

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*Essai sur la Statistique de la Population Francaise. Par le Comte A. D'Angeville.

cially when that life is twice as great as in some of the cemeteries of Boston? The foreign population of Boston is about equal to the native. Nearly all of the former are Irish. These Irish, in going from their own country to Boston, go to a climate like their own, not to a tropical climate, as in New Orleans—an important consideration.

Now, the Report shows in the Potter's Field, or Bayou Cemetery, [the great Irish necropolis of New Orleans,] that the average life taken for six years, ending in 1846, is nearly twenty-four; that is, nearly one-fifth more than the native and foreign mean life of the most favored Northern cities.

The Report estimates the mortality for 1849 at 9,862, of which number Louisiana and New Orleans furnished less than one in every twelve, and the residue of the United States about one in twenty. Those known to have been foreigners, [3,569,] and those whose places of nativity were not known, that is, 4,985, were probably nearly all foreigners, negroes excepted. The United States, including Louisiana and New Orleans, contributed to the whole number, namely, 9,862, only 1,308. Now, let us give the Report nearly twice as many deaths for the United States, namely, 1,017 more than the number mentioned in the Report, whereby it will appear that foreigners, nevertheless, exceeded Americans between four and five times, or, by the figures of the Report, nearly eight times. Can any other city show a mortality in which *its own share*, including that of the temporary residents of the whole State, is less than one in twelve? Concede to the Report 1,017 deaths of native Americans beyond its own account—admit [what is probably true] that all the centenarians mentioned are natives, and what is the result? The Report gives one aged 130, one aged 110, one aged 105, and twenty aged 100, or twenty-three aged 100 and over; that is, more than one centenarian to every 101, or 2,500 times more than France; or, taking the deaths of Louisiana alone, twenty three centenarians in twenty-nine deaths, or one centenarian to thirty-three deaths among the natives of the city.

Even the cholera passed by the natives of New Orleans to a very great degree, taking the Report as authority. Thus, of 3,171 deaths which took place from that disease in 1849, the natives of the city and State contributed only

106; or 1 in 30 nearly of the whole. Among 783 deaths from yellow fever, the State and city contributed 2, or 1 in 391.5, and among 640 deaths from all other fevers, but 17, or about 1 in 40.

The mortality of 1849, though great, illustrates the mortuary statistics of New Orleans for other years. Dr. Fenner, in his Medical Reports, shows that in 1849 the total number admitted into the Charity Hospital of New Orleans was 15,558—of these 13 634 were from foreign countries; from countries unknown, 142, probably nearly all foreigners, making 13,776 foreigners; from the different States of the Union 1,782 were admitted, leaving, as he shows, only 147 for all Louisiana; that is, one Louisianian for every 10,397 patients born in other climates.

Dr. Fenner shows that of the whole number admitted into the Charity Hospital in 1849, 17½ per cent. died, making 2,739 deaths. This ratio will give Louisiana about 27 deaths—but several reasons could be adduced rendering it probable that Louisiana did not furnish an average mortality half as great in the 100 as foreigners —say 12 in 147. Here, then, are 12 deaths in 2,727, or if figures be taken without explanation, 147 sick Louisianians furnished, in 1849, no fewer than 2,739 deaths.

Dr. Fenner gives the statistics of the Charity Hospital for twenty years, ending in 1850; admissions, 123,917; of these 1,283 were Louisianians, a little over one in a hundred. Adopting the usual estimate, namely, that the other hospitals and the private cases in the city exceed those of the Charity Hospital in the ratio of two to one, then the last twenty years gave 321,917 cases of sickness, of which 3,879 were from Louisiana—in round numbers, 3,000 to 318,000.

Of 1,800 who died of yellow fever in New Orleans in 1841, the State of Louisiana and its cities contributed but eight, or 1 in 225; the nine most Southern States, including Texas, only twenty-five, or one in seventy-two; and the entire black race only three, or one in six hundred.

The hospitals of Paris are for Frenchmen; the Charity Hospital of New Orleans, the only one in the State of Louisiana, is virtually for foreigners. In Paris one-sixth of the whole population die in the public hospitals.* In

*Dupin Alison.

a population of 70,000, or one in every ten, pass annually through the public hospitals,* while in the Charity Hospital of New Orlhans, the whole State, in twelve years, ending in 1842, supplied, among 59,021 patients, only 556, or 45 annually—that is, one to ,731—a ratio 783 times less than that of Paris. In 1842, among 4,404 patients in the Charity Hospital, Louisiana furnished only 34, not one in ten thousand of the inhabitants, or one thousand times less than Paris. In Dublin, in 1827, more than one in four entered the fever hospitals of that city, namely, 60,000†—a ratio twenty-five thousand times above that of Louisiana.

ART. VI.—*The East Tennessee Record of Medicine and Surgery.* Edited by FRANK A. RAMSEY, A. M., M. D. Knoxville: 1852.

Dr. Ramsey, the editor, has shown himself worthy of success in this first number of his journal. It bears the most unquestionable marks of industry and ardent professional zeal. He has our best wishes—we wish him all he expects or hopes from his enterprise, and would not add a word to damp his praiseworthy ardor. A large part of this number is from the pen of the editor. His report on the diseases of East Tennessee is an elaborate paper, and his editorials are spirited and manly.

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*Ib. †Alison.

SELECTIONS

FROM

FOREIGN AND HOME MEDICAL JOURNALS.

Cases treated with Veratrum Viride. By Dr. D. C. O'Keeffe. (Reported by J. S. Clements, of Penfield, Ga.)

Case I. Nov. 3, 1851.—Mrs. ——, aged 21, was delivered of her first child one week ago; progressed satisfactorily until above date. Condition at that time: Skin hot and dry; tongue furred, white on centre, red at tip and edges; mouth dry and lips crusted; slight headache; pain in back and bowels; least pressure on abdomen causes pain; pulse 115, small and hard; dysuria for last twenty-four hours; has had two stools during same period from castor oil and spts. turpentine she had taken. *Diagnosis*—Incipient puerperal fever. *Prescription:* Pepper poultice to abdomen, warm cloths to vulva, spts. nitre in parsley tea every two hours, until dysuria is relieved.

5 o'clock, P. M. Has urinated tolerably freely since morning, less pain in bowels, pulse 120; other symptoms the same. *Prescription;* Eight drops tinct. veratrum viride every three hours until pulse is reduced.

8th—9 o'clock, A. M. Took but two doses of the veratrum viride, before nausea was induced, and lasted two to three hours. Simultaneously with the induction of nausea, perspiration set in, the pulse was lessened in frequency, and continued so all night. Present condition—Pulse 100, skin cool, tongue less red, feels but little pain and sleeps

better, except during existence of nausea, than she had done in several nights. Took a dose of oil and turpentine at 1 o'clock this morning, which has not operated yet; feels nausea occasionally.

9 o'clock, P. M. One stool; feels better, nausea occasionally all day; pulse 85; has taken none of the veratrum viride since last night. Ordered 10 gtt. tr. veratrum viride every three hours, till nausea occurs, if fever returns. Has passed urine tolerably freely to-day.

8th. Has had no fever of consequence since last date, and therefore needed no medical attention.

REMARKS.—This case would, in all probability, have terminated in puerperal fever, of which it presented the essential characters, but for the timely administration of hellebore. The febrile action could not have been owing to the dysuria and consequent distention of the bladder, for after these symptoms were relieved, the pulse numbered 120; neither could it be fairly attributed to want of action from the bowels, for they had been moved twice the day the first note was taken. She had had her "milk fever" three or four days before the date first noted, and got on well during the interim; the lochial discharge was normal, and the untoward features were attributed by the family to "a cold she had taken." It is reasonable, therefore, to infer that it would have run its course, as a case of puerperal fever, had not the potent agency of the veratrum viride curtailed its progress. This is the first recorded case of puerperal fever treated by the American hellebore, and the sanguine expectations of its worthy pioneer, Dr. Norwood, have been fully answered.

Case II. Nov. 8th, 1851.—Eliza, a negro woman, aged 25, strong and vigorous constitution, was taken with pneumonia of right lung on the 4th; was seen and treated by Dr. A. A. Bell until the 8th, after the usual manner. Calomel, tart. antimony, ipecac, Dov. powder and blisters, constituted the treatment. He had used the tinct. of veratrum viride freely for two days, without producing any perceptible effect whatever, in addition to the remedies above enumerated. The patient's condition at above date was as follows: Skin warm and moist, from ipecac and tart. antimony she had been taking freely all day; pulse 120–130, small, compressible under influence of tartar-emetic; bowels disposed to looseness, and tender to the

touch. At 5 o'clock, P. M., she took 10 gtt. tinct. veratrum viride to control the circulation, which had not been influenced in the least by the calomel and tartar-emetic which she had been taking for twenty-four hours.

7 o'clock, P. M. Pulse the same; still perspiring—15 gtt. veratrum viride. 8 o'clock, P. M. Nausea and profuse perspiration; griping in bowels, which was followed by a copious watery operation; pulse 110—took 60 gtt. tinct. opii. to check bowels. 9 o'clock, P. M. Nausea and perspiration the same, bowels easy, feels drowsy, pulse 100, intermittent. 12 o'clock at night. Nausea and perspiration, pulse 100, not intermittent—took 10 gtt. veratrum viride.

9th—10 o'clock, A. M. One operation since last note, followed by 30 gtt. tr opii; still perspires; skin cool; pulse 100, not intermittent; no pain; feels drowsy. Ordered, 10 gtt. veratrum viride every three hours while the pulse numbers 100—should it fall before that, to be given less frequently. Convalescence commenced at above date.

Remarks.—This case Dr. O'Keeffe saw in consultation with Dr. Bell, who had judiciously treated it with the usual remedies. He had even given the hellebore a fair and impartial trial for two successive days; but it failed —signally failed. The present case was the second in which the incomparable remedy, hellebore, failed in Dr. B.'s hands to produce any effect whatever, and he was on the eve of condemning it as an insignificant puff, when a different preparation proved to him satisfactorily that his was an inferior article—was inert. Should this disappointment happen to others, they would do well to pause, and consider the reliability and intelligence of their druggist. Dr. B. obtained his tinct. of veratrum viride from his druggist, while the preparation that fulfilled its mission—told the heart's arteries "so fast shalt thou beat and no faster," was prepared by a physician, in his own office, in the proportion of two ounces to the pint of alcohol.

It is difficult to say, with unerring certainty, what agency the veratrum had in bringing on a favorable crisis in this case, or how it would have terminated under the usual treatment. She took the first dose of hellebore at 5 o'clock, P. M., the tongue being moist and skin warm, but moist also; notwithstanding these favorable effects

from the calomel and tart. emetic, the pulse was not reduced any. Two doses of the hellebore accomplished what several doses of the tart. antimony had failed to do. A crisis might have taken place without the veratrum, and its occurrenee, after its exhibition, may have been a coincidence instead of an effect; but from the infallible certainty with which it reduces the action of the heart and arteries, I think it not unreasonable to attribute the favorable change, in the present case, to its potency.

Case III. Mr. B., aged 45, complained of cold chills and acute pain in left side on 12th of November. Poultices, baths, diaphoretics and expectorants, failing to relieve him, he was bled next day 20 oz., the pulse being 100, full and strong. The v. s. and perspiration that followed relieved the pleuritic pain, partially, for a time, but it returned next day with some severity. A blister was applied to the affected side, which removed the pain; took 15 grs. of calomel, twice, in divided doses, which produced three dark operations. During this time, the tongue was covered on the middle with a whitish fur, natural at edges; the mouth dry, clammy, and exceedingly unpleasant, required cleansing frequently in the day; the pulse during the whole time 90–100.

8th day of illness. Skin hot, harsh and dry; tongue furred, clammy and sticking to roof of mouth; taste unnatural, very feeble and extremely restless; pain no where, pulse 90 to 100, variable, feeble and hard. Took several doses of diaphoretic preparations, until altering the condition of the skin, which was so hot and dry that he said he felt like he should "burn up." At 10½ o'clock, A. M., took 10 gtt. veratrum viride, the pulse being 100. At 11 o'clock, the skin was warmer and disposed to become moist; pulse stronger and more frequent, 114; no nausea. 1 o'clock, P. M. Skin moist, generally; large drops of sweat on face, drowsy, no nausea; pulse 114. 3 o'clock. Vomited considerable quantities of bilious matter several times. After he had vomited twice, skin became cool and dry, and the pulse fell to 100; and after he had vomited two or three times more, the pulse fell to 80, was small and weak. 5 o'clock. The vomiting having continued, and feeling griping pain in lower bowels, he took two tea-spoonfuls of paregoric, which checked the vomiting and griping. Expectoration, of which

there was little or none before nausea, became profuse after it, and relieved the lungs very much. He took no more of the hellebore, and his pulse fell to 60, and continued so for several days, when convalescence commenced, and progressed satisfactorily to recovery.

Remarks.—Here is a case in which the usual diaphoretic remedies had been used freely and faithfully, without effecting the desired result; whereas 10 gtt. of the veratrum acted like a charm. The hellebore was not given in this instance to diminish the frequency of the pulse—for that was not at all imperative—but to act simply as a diaphoretic, and to remove the great heat of which the patient complained so much. It is proper to note that, before nausea set in, and after taking the veratrum, the temperature of the surface was heightened, and the pulse accelerated from 100 to 114 in the minute—a result that ensued only in the present instance.

Without further notice of this case, which was considered one of typhus fever, I propose to give, in detail, the notes of the case of pneumonia in which the hellebore was administered as a principal remedy during the whole progress of the disease.

Case IV. Nov. 23d, 1851, 9 o'clock, P. M. A negro girl, aged 20, of strong constitution, in seventh month of pregnancy, felt pain (slight) for the first time, in her left side, this morning, but attended to her usual duties until three hours before she was prescribed for. Condition: acute pain in her left side; cough hard, painful and frequent; skin hot and dry; tongue clean; pulse 120, and strong. *Diagnosis*—Pleuro-pneumonia.

Prescription: Venesection 1 quart; poultice to left side, and 10 gtt. tinct. veratrum viride. 12 o'clock. The same; 15 gtt. veratrum viride. 3 o'clock, A. M. The same; 20 gtt. veratrum viride.

24th, second day, 7 o'clock, A. M. Skin a little cool, vomited some since last note, perspired some during nausea; other symptoms the same—23 gtt. veratrum viride. 8 o'clock, A. M. Nausea and vomiting several times, skin natural, pain almost relieved; pulse 80, small and weak; deathly sick—Ginger tea to relieve nausea, and poultice to chest. 6 o'clock, P. M. Took the veratrum every three hours since last note; skin warm and dry; one stool; pain in side returned; pulse 112—15 gtt. ver-

atrum viride. 8 o'clock. Vomited three times; skin moist; pain in side the same; pulse 80. ℞. Calomel, Dov. powder, *aa* 5 grs.; ipecac, 3 grs.; to be taken at once —cups over seat of pain, and poultice.

25th—9 o'clock, A. M. One stool; emesis several times in night; skin harsh and dry, but natural in temperature; cough hard and painful; pulse 120—15 gtt. veratrum viride every three hours, poultice to chest, and expectorant to relieve cough. 7 o'clock, P. M. Took three 15 gtt. doses of the hellebore, after the last of which she vomited three times; no stool; skin cool; pulse 80; respiration rapid, 50 in the minute. 11 o'clock, P. M. Took no medicine since last note; respiration rapid, embarrassed and difficult; skin cool, but harsh and dry; very restless, pulse 120, hard and constricted—15 gtt. veratrum viride in 50 gtt. tr. opii., to produce sleep.

26th—1½ o'clock, A. M. Took since last note 15 gtt. veratrum viride: respiration and pulse the same; skin warm and moist, is in a copious perspiration; no nausea —20 gtt. veratrum. 8 o'clock, A. M. Respiration and pulse the same; skin dry and warm; no stool; pulse 120 20 gtt. veratrum viride. 9 o'clock, A. M. Was sitting up when last note was made, which perhaps accelerated the pulse somewhat; skin a little moist and warm; pulse 100, lessened 20 beats without any nausea or vomiting; "felt worms creeping up her throat," attended with a flow of saliva and a desire to heave. (Complained of this sensation of worms in the throat since she has been taking the hellebore, and the same feeling was described by another patient who was taking the veratrum at the same time.) 15 gtt. veratrum viride every three hours, and intermediately a powder of calomel, Dover's powder and ipecac; blister to left side. 7½ o'clock, P. M. Took the above; no stool; after second dose of veratrum, vomited freely; skin warm; perspired none; blister well drawn; cough loose; pulse 90—continued veratrum every three hours, and gave injection to relieve bowels.

27th—9 o'clock, A. M. Two stools; emesis several times; cough very troublesome, causing vomiting; respiration very rapid, 50–60; pulse 100—15 gtt. veratrum viride, and same powders intermediately; blister dressed.

28th—8 o'clock, A. M. Followed prescription; two stools; constant nausea and vomiting; cough hacking

and troublesome; tongue slightly furred; respiration same; pulse 100—continued hellebore and powders. Night visit: No improvement; felt labor pains since last note. *Prescription:* Discontinued hellebore; blisters to legs and right side; quinine, Dov. powder and ipecac, every three hours, and tart. antimony alternately.

29th—9 o'clock, A. M. Miscarried: pulse very frequent; getting worse—same treatment continued; brandy and quinine if pulse should sink. Night visit: Dead.

Remarks.—This case of pneumonia tested the hellebore to its utmost capacity, and brings home to our mind the very reasonable conviction that it will not prove infallible in the treatment of pneumonia. On reviewing these notes, it will be seen that the heart and arteries seldom failed to respond to the sedative influence of the veratrum, while the respiration was not affected by it in the least; it was a singular phenomenon to observe the respiration as frequent as 50 or 60, the skin cool and moist and the pulse 80. Hence, in this case, at least, it did control the circulation, but did not affect the respiration or inflammation. There is every reason to believe that the pneumonia inflammation received no check whatever, notwithstanding the circumstance that the patient was kept fully and constantly under the influence of the hellebore. But it is a question of reasonable doubt, whether any course of treatment would have averted the inevitable doom that awaited this patient in view of her advanced state of pregnancy. It is the duty of physicians who submit this agent to the test of clinical experience, to watch carefully its effects, and report them for the benefit of the profession: the remedy is in a state of probation preparatory to its admittance into the sanctum of practice, and nothing should be withheld that may add to its value or detract from its merits. In view of the severity and well marked character of the diseases in which it is most likely to be used, its effects can be early determined, either as positive or negative: it will control the bounding pulse, or it will not; there is no half-way ground—no room for speculative propensities. In a type of typhoid fever, in which pulmonic symptoms predominated, Dr. O'K. assures us it had the happiest effects in curtailing the duration of the fever and expediting convalescence. But in cases in which inflammation of the intestinal mucous membrane

exist, its exhibition may prove somewhat questionable. It may be said, that as far as the cases go, the claims of the American hellebore to a potent remedy are founded on no fictitious representations: they rest on the indisputable facts of rigid experience; and we have little doubt that when it shall have passed through an extensive trial, the conviction of its importance will become universal, and in the language of Dr. Norwood, a new era in the treatment of disease will be the consequence.—*Southern Med. & Surg. Journal.*

Veratrum Viride, as a Poison. By E. M. Pendleton, M. D.—Having recently had the opportunity of trying the Tinc. of Veratrum Viride, (Dr. Norwood's preparation, purchased from himself, personally,) I feel it my duty to give a brief account of its therapeutical effects, as I would gladly have done, had the impressions made upon my mind been of a more favorable character. Ever loth to try new and potent remedies, as I abominate *experimentalism* in medicine, where danger may be apprehended, I had thus far resisted the temptation of using the article in question, notwithstanding the high character it had attained in the hands of Dr. Norwood and others, as has been made known in the Medical Journals. On the day in question, however, Dr. Norwood called at my office, and presented such a "cloud of witnesses," in the shape of letters received from divers physicians, some of whom I knew, that I was induced to take an ounce of his preparation, promising to give him, or some of the journals, the result of my experiments.

At noon of the same day, I called to see a patient laboring under Catarrhal Fever, whom I had bled in the morning to syncope, having had a remarkably hard and intractable pulse. She had been sick about a week, having the usual symptoms of Catarrhal or Inflammatory Fever—soreness in the muscles, pain in the back and head, red, watery eyes, and cough, with mucous expectoration. I found her pulse had rallied again, being very hard and bouuding, and going at the rate of 133 beats to the minute. A first rate case, thought I, for the Veratrum Viride. Accordingly, I prescribed Dr. Norwood's minimum dose,

eight drops, pouring it out with my own hands, leaving directions for her to take another portion in three hours, the time designated by the Doctor.

About six, P. M., two hours after she had taken the last dose, I was sent for in great haste to see my patient, who was thought to be *moribund.* I was at her bed-side in a short time, and found her laboring under excruciating pain in the precordia, with a sense of suffocation and sinking at the stomach, cramps throughout the muscular system, which assumed a periodical character, a preturnatural coldness of the whole system, with clammy sudor. Her eyes were wild, pupils dilated, countenance expressive of great pain and apprehension, which were evidenced by her plaintive cries for help. She sometimes spoke incoherently, showiug a wandering mind, and a state bordering on the destruction of nervous vitality. Her pulse was scarcely perceptible, and numbered 120 to the minute. I of course felt great alarm, and set to work to keep up the sinking powers, by warm mustard poultices and frictions externally, with wine and brandy internally. Notwithstanding all my efforts, the pulse became entirely imperceptible, and all the symptoms above described assumed a more alarming aspect, in connection with copious watery evacuations *per anum*, I ordered starch and laudanum injections, and persisted in the stimulants, etc., until about 8, P. M., two hours after my arrival, when her pulse again became perceptible, which was so weak that it reminded me more of the delicate thumping of the fetal heart on the tympanum in a stethoscopic examination four months after conception, than any thing else. What was remarkable, however, it was only 84 beats to the minute, showing at once the paternity of all this suffering and danger, as well as the potency of the drug administered, "for evil, however, rather than good." All the other unpleasant and deathly symptoms began to give way, as the heart which had been so suddenly crushed in its nervous energies, resumed its wonted sway. None but the truly conscientious physician, who holds himself accountable, not merely at the bar of public opinion, but at the bar of God, for all his professional, as well as other deeds, can appreciate my feelings at this termination of the case.

I found my patient, early the next morning, greatly re-

lieved, pulse 96, full and strong. It continued to increase in frequency and strength until it reached nearly the condition it sustained prior to the administration of the Veratrum Viride. I of course resorted to another and safer class of remedies for its subduction, which resulted in a favorable termination of the case.

I have thus, in all candor, presented the results of my experience with a drug, whose sedative and poisonous qualities I regard but little if any inferior to the Prussic Acid, and decidedly more potent than the Digitalis. . It is true only one case is presented, but one so clear and well marked. that none can attribute the result to other causes than the drug itself. Whatever effect the relation of this case may have upon the uninitiated, I must confess upon me it has had the effect to make me abandon the remedy, at least for the present. Bolder practitioners may, in this, as in other instances, educe good from evil, and free the remedy from some of its more poisonous principles; but our present knowledge of it will not warrant an indiscriminate use of it, without rendering us culpable of a violation of some of the plain laws of humanity and professional propriety, not to say of religion. —*Charleston Med. Journal.*

The Itch Cured in Two Hours. By Dr. Hardy, (London Lancet,) Dr. Bazin, physician of the Hospital Saint Louis of Paris, introduced, not long ago, a notable improvement in the treatment of the itch, since he succeeded in curing the disease in *two days* by general frictions with the sulphur ointment. Dr. Hardy, who has succeeded Dr. Bazin in the Scabies wards of the same hospital, has, however, considerably curtailed this already short time; he cures his patients in *two hours.* The method is described as follows: Patients are no longer admitted into the house for the treatment of the itch, as two hours suffice to render contagion impossible and the recovery almost certain. The patient is put into a warm bath, and rubbed for an hour in yellow soap; he then passes into a clean bath, where he continues to cleanse his skin for another hour. After leaving this bath, he is taken to a particular room fitted for the purpose, and, with the aid

of one of his fellow-sufferers, he is rubbed all over for half an hour with the following ointment:—Axunge eight parts, flowers of sulphur two parts, carbonate of potash one part. After this friction, the patient is examined and sent away cured, though sometimes pretty numerous vesicles on the hands and elsewhere remain unaltered. Dr. Hardy states that out of one hundred cases, he has only had two or three relapses. The number of itch patients has considerably diminished, as none are now turned away for want of room; and the disease has thus spread with much less rapidity.—*Ibid.*

Treatment of Obstinate Ulcers by the Internal Use of Tincture of Cantharides.—By J. Tart.—(Prov Med. and Surg. Jour.) In a case of extensive ulceration in a broken constitution, after the failure of various plans of treatment, Mr. Tart gave ten drops of tincture of cantharides three times a day, with marked benefit. In three days from the commencement, the sores began to contract, healthy lymph appeared round the edges, and vivid granulations started up. In a fortnight, the ulcers were quite healed. On this case the author remarks:

"Such was the progress and issue of a case that had baffled every previous treatment employed. It affords one of many examples I could bring forward of the great utility of cantharides in indolent ulceration, dependent either upon atony of the engaged parts, or system generally.

"In 1845, while resident in Burmah, my attention was directed to the treatment of the ulcers met with in that country, and which had long been found difficult to heal by different medical gentlemen stationed on that coast. I drew up a paper, exhibiting the appearances presented by the different ulcers, and the states of constitutional derangement with which they were identified, and in which I had employed the tincture of cantharides with marked success. The paper alluded to, backed by several cases treated by different medical friends, was forwarded to the Madras Medical Board, who ordered it to be circulated throughout the medical service of the Madras army."

A few extracts from the paper here referred to will show the characters of the ulcers where I found the tincture of cantharides useful:

1st. "Where the granulations were exuberant, but pale, weak and flabby.

2d. "Where there was deficiency, or total absence of granulations, the ulcers being deep and scoped out, with raised and indurated edges.

3d. "Where the granulations were not defective, but cicatrizing irregularly, sometimes in the centre, at other times on one side, the lymph which was thrown out and organized one day being absorbed the next.

"It was further remarked that their indolent character was to be referred to morbid states of the system, varying from little perceptible deviation from health to the different grades of anemia. It was difficult at first to assign the cause of this indisposition to heal, in sores which presented themselves in subjects where the general health was little impaired, and this was increased on finding that they did not yield to the exhibition of remedies known to exercise a beneficial influence upon such ulcers. It was from observing the ineffective attempts at reparation, which I occasionally witnessed in the different cases that came before me, that suggested the belief of the primary derangement to be owing to deranged circulation of the affected part, and the blood being deficient of, or not elaborating, plastic lymph of sufficient quantity or quality necessary for the healthy granulating process.—*Ibid.*

On the Use of Full Doses of Opium in Continued Fevers. By Dr. Haycraft.—(London Lancet.) Dr. Haycraft considers that the administrntion of opium, in the latter stages of continued fever, is a practice which has fallen into undeserved disuse, which, he remarks, is the more unaccountable, inasmuch as the usual effect of fever is to deprive the patient of sleep. He argues that, if any one were to be deprived of sleep for ten, twelve or fifteen days, he would be, by that time, in a state somewhat similar to that of the exhaustion of fever.

"By what train of reasoning (he asks) can we come to the conclusion that sleep is less necessary in disease than

in health? That the occurrence of sleep is highly salutary in fever requires for proof (Dr. Haycraft adds) only the concurrent testimony of all observers, provided that the sleep be not of the comatose kind. Even the nurses of our public institutions will frequently form a favorable and correct prognosis, when they say, in hospital phrase, "the patient is sleeping an end," and the event seldom falsifies the prediction."

Our author examines into and comments on the opinions of Sydenham, Van Swieten, Boerhaave, Friend, Richter, Cullen, Mason Good, Potter, Armstrong, etc., as to the employment of opium in fever. He remarks that both Sydenham and Van Swieten mention delirium as the symptom which indicates its use; but that, from most of the authors above named "placing this drug at the *end* of their lists, it might fairly be implied that it is only to be given at the termination of the disease—certainly not at its commencement."

The presence of inflammation is far from contra-indicating the use of opium in fever. Drs. Stokes and Graves have shown that, in peritonitis, the inflammation may be subdued by the use of opium alone, even when the intestine has been perforated.

Some of the conclusions of Dr. Stokes, as quoted by Dr. Haycraft, are, that where antiphlogistic measures are inadmissible, the administration of opium in large doses may be made with benefit, and that its effect is then to raise the vital power. Our author believes that this effect may be "partly due to the production of sleep, which, however induced, must act beneficially on depressed vitality; that it should subdue irritability may, I think, (says Dr. Haycraft,) be fairly attributed to the same operation."

Dr. Haycraft lays great stress on the injunction, that the remedy should be administered in *full* doses. A small dose, as he avers, will only irritate, and therefore increase restlessness and delirium; and the later opium is used, in the progress of the fever, the more decided, he considers, will be its beneficial effects; if given on the fourteenth day from the attack, it will, he says, usually produce a most decided critical sleep: "the fever is actually cut short." Earlier than the eighth day, he states that he has, from the nature of his practice, not had opportunities of

giving it; but he would not hesitate to employ it after the sixth day, provided *restlessness*, *sleeplessness*, or *delirium* were present. This treatment of fever by opium is not, however, he cautions us, to supersede other remedies, which may be specially required to combat concurrent symptoms—wine, brandy, diffusible stimulents when exhaustion takes place, sulphuric acid in large diluted doses, when colliquative sweats exist, or a scruple of calomel, with or without opium, when the congestion of collapse occurs in the brain, lungs or abdomen. He does not consider opium, in suitable doses, a remedy in anywise contra-indicated for children suffering from fever.

Dr. Haycraft cites several cases, which have occurred in his practice, illustrative of his views. The following are the conclusions at which he has arrived:

"1st. That opium may be safely given after the sixth or eighth day of a continued fever.

"2d. That it will be most beneficially prescribed when restlessness, insomnolentia, delirium or sinking takes place.

"3d. That a drachm of the tincture of opium, or an equivalent quantity of any other preparation of opium, is not too large a quantity for an adult.

"4th. That opium, being an antiphlogistic remedy, is not contra-indicated by inflammation.

"5th. That opium succeeds best after purgatives—that if these have been neglected it should be joined with a purgative; if the symptoms urgently require its use, it may be combined with diuretics.

"6th. That opium, given in the latter stages of fever, in sufficient doses, raises the vital powers, abates irrita-

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Anesthesia.—The British and Foreign Medico-Chirurgical Review, for January, gives the following summary of the deaths from anesthesia, in the order of the occurrence of the cases. The list does not include several cases which have occurred within the last eighteen months, nor, as the writer remarks, "at least five well-authenticated deaths from needless and careless use of chloroform in

this country (England) alone, or of numerous cases where dangerous symptoms have been observed, or death obscurely traced:

"1. Hannah Greener, aged fifteen, was greatly afraid of respiring the chloroform,—only about two drachms were used,—insensibility was produced in half a minute, when the removal of a great toe-nail was commenced; death occurred in about two minutes.

2. Case at Cincinnati, aged thirty-five, apparently in good health; was chloroformed for about a minute for extraction of a tooth; death occurred in from five to ten minutes.

3. Patrick Coyle, chloroformed for fistula; he inhaled for about a minute, and almost instantly expired.*

4. Mdlle. Stock, Boulogne, aged thirty, subject to palpitation, and chlorotic; inhaled about fifteen or twenty drops; operation, opening of an abscess in hip; death almost instantaneous.

5. Daniel Schlyg, aged twenty-four, had a thigh fractured by a ball during the days of June, in Paris, and was in a state of profound depression; he inhaled the chloroform for about three minutes, amputation at the hip-joint was performed, and death occurred in three quarters of an hour.

6. Walter Badger, aged twenty-three, had heart and liver disease; he inhaled chloroform for about a minute, previous to the extraction of a tooth; the operator, Mr. Robinson, of London, was absent for less than a minute to seek more chloroform, and found his patient dead on his return. The inhalation took place from an apparatus.

7. A young woman of Hyderabad was chloroformed for amputation of middle finger of left hand; about a drachm of chloroform was used; death almost instantaneous.

8. John Griffith (Warren) had chancres and hemorrhoids; inhaled about three drachms, and died in about ten minutes, during the excision of the hemorrhoids.

9. Abbey Pennock (Warren) inhaled about three drachms in two applications, to relieve the pain of toothache, and died almost immediately after the second application.

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*The case of Coyle is given by Dr. Warren alone.

tion, subdues inflammation, and most frequently induces a sleep, which becomes truly the crisis of the disease."—*Ibid.*

10. Example by M. Malgaigne. A man, one of the wounded of the three days of June, had the humerus broken by a ball, and was a good deal weakened by hemorrhage and gangrene of the wound; was chloroformed, and the humerus disarticulated; new inhalations were taken during the search after the ball, and he died during the last incision.*

11. Charles Denoyers, aged twenty-two, was affected with white swellings of the left wrist, and chloroformed at the Hotel Dieu, of Lyons, for five minutes before and during cauterization of the tumours; death took place shortly after the commencement of the operation.

12. Case of M. Roux; removal of a scirrhous breast; death before quitting the amphitheatre.

13. Case described by M. Guérin, as having taken place at the Bicêtre. A man affected with a lesion of the thigh was chloroformed for amputation at the hip-joint; death took place before the end of the operation.

14. Case at Govan; a young man chloroformed for removal of a great toe-nail; death almost immediate.

15. Case of M. Barrier, of Lyons; a young man, named Verrier, aged seventeen, inhaled from six to eight *grammes* of chloroform for about six minutes, for an amputation of the finger, and died in about half a minute afterwards.

16. At the Hospital of Madrid, (Bouisson,) a child of twelve years being chloroformed for an amputation of the leg, was seized with a tetanic spasm, and died in a minute and a half.

17. By M. de Confevron, of Langres. Madame Labrune, aged thirty-three, of a nervous temperament, was chloroformed for the extraction of a tooth; almost instant death.

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*Dr. Warren gives 10 fatal cases, but inserts the cases which we have omitted for the reasons above given. This last case is our 9th; and our 11th is the 7th of M. Bouisson, who, like us, excludes suicidal cases, and such like. He gives 15 cases; his last is our 17th.

18. Mr. Solly's case; a man, aged forty-eight, apparently in perfect health, inhaled rather more than a drachm of chloroform from an inhaler for about three minutes; then a toe-nail was removed; and death took place in about six or seven minutes from the beginning.

19. Case at Leeds, Robert Mitchell; chloroform applied during an attack of delirium tremens by Mr. Teale; death about an hour afterwards. (Hardly a clear case.)

20. Case at Shrewsbury; a girl named Jones; only a small quantity of chloroform was given before proposed extirpation of the eyeball; almost instant death, as if from apoplexy.

21. Case at Berlin; a young lady died during the extraction of a tooth, from the alleged effects of about half a drachm of chloroform.

22. Case at Guy's Hospital, referred to in 'Med. Gaz.' for June, 1850.

23. Aschendorf's case; a child of one year old operated on for a nævus under the influence of chloroform; only about nine drops were used; death on removal from the table.—*British & Foreign Medico-Chirurgical Review.*

On the Action of the Ergot of Rye, &c.—Dr. Arnal, in the *Memoirs of the French Academy of Medicine*, has some interesting observations on this subject, which we find quoted in the Brit. and For. Med. Chir. Review. As the result of extensive trials, he states that the *aqueous extract of the Secale Cornutum* possesses great hemostatic powers in internal hemorrhages. From numerous trials on poultry as well as the human subject, with every variety of dose and preparation, he comes to the following conclusions:

1. The ergot of rye contains a poisonous principle, productive of death, but by no means so energetic as usually represented.—2. Given in the entire grain it acts much less energetically than when powdered.—3. Recent ergot does not act more efficiently than older; but, on the contrary, this last is sometimes the most active of the two.

In order to produce a *summum* of the effect, it is necessary for it to undergo, in the vessels in which it is kept, a peculiar change, which softens it, and imparts to it an odour *sui generis*. Thus it should not be ordered to be powdered just before using.—4. Much greater effect is produced by a certain quantity, in fractional doses, than when given only at twice, probably because less escapes the influence of the digestive organs; one of the effects of divided doses, is to produce a loss of feathers; but in all his numerous experiments, both with large and small doses, Dr. Arnal has never met with anything analogous to the dry gangrene, said to be produced by ergotism in man; but which, seeing that ergot exerts a fluidifying effect upon the blood, he is disposed to attribute to other causes. —5. The ethereal oil of ergot has not proved fatal in his experiments as it did in those of M. Bonjean, and he attributes the issue of these latter to the fluid having entered the air-passages, when it proves rapidly fatal.—6. The watery extract does not contain poisonous matter, or it does so in such small proportions as to prove injurious only after prolonged use. The toxical principle thus insoluble in ether or water, is found in the residue, which kills animals just as the ergot does.—7. The ergot, however given, is very slow of digestion; and when given in excess, it produces lesions of the digestive organs. Some of these are found on post-mortem examination to resemble precisely those observed in typhoid fever, and the author exhibits a parallel of the symptoms of typhoid and poisoning by ergot.—8. The ergot modifies the composition of the blood, rendering it more diffluent; and if exhibited long enough in divided doses, it will induce all the symptoms of scorbutus. Nutrition especially suffers from its deleterious action, as is seen by the rapid emaciation that takes place in the animals to which it is given. The aqueous extract exerts a much less modifying power upon the composition of the blood, than do the other preparations.—9. The ergot, in experiments made upon man, reduces the pulse by several beats for some hours; but even by repeated doses, Dr. Arnal has never known these reduced lower than forty-eight, even in the aged.—10. The beneficial effect which ergot exerts upon uterine hemorrhage, has led many to believe that its action is elec-

tive, as regards the uterus; but in thirty cases of other internal hemorrhages, in which the *aqueous extract* has been administered by the author, a cure has been effected, or, when the presence of organic disease prevented this, amelioration has been procured. It is, however, not so applicable in all forms of hemorrhage as in uterine. It is rare for active, idiopathic hemorrhage to resist its action for more than twenty-four or forty-eight hours; but when this has become passive, the remedy may even prove mischievous if it be continued too long, or the dose be too large. It is also inefficacious in subjects originally feeble, or exhausted by protracted disease. Even in subjects of good constitution, when given too long in large doses, it may produce bleeding of the gums, and an injurious depression of the circulation. In hemorrhage symptomatic of organic lesion, the ergot acts as a hemostatic, but cannot prevent the return of the bleeding. Yet in the case of hemoptysis, dependent upon tubercle, it may act beneficially, not only by suspending or moderating the *molimen hemorrhagicum*, but also by moderating the inflammatory action of the portion of lung surrounding the tubercular deposit. In the same way, it has proved of constant service in acute bronchitis; and in pneumonia it has rapidly suppressed bloody expectoration, and moderated other symptoms. So well does the author think of it in this point of view, that when the patient's strength requires husbanding, and the pneumonia is not too extensive, he recommends commencing the treatment with the ergot, which, by its deoxidizing agency on the blood and retarding power over the heart's action, is an antiphlogistic, *par excellence;* the debilitating effects which attend other means being not produced by it, or if they should present themselves, ceasing on the discontinuance of the remedy. M. Arnal believes that the experiments of arresting traumatic hemorrhage by the local application of the extract, so favorably reported on by M. Bonjean, require repetition and extension to larger vessels.—11. Ergot in its native state is more active in its operation, but its watery extract is less dangerous.—12. M. Arnal takes the present opportunity of confirming the favorable accounts he formerly gave of the utility of the extract in *chronic engorgements of the uterus.* Some of these cases,

however, require a prolonged perseverance in the use of the remedy.

Eighteen cases of hematemesis, epistaxis, &c. &c., are related in illustration. The following is the formula prescribed: Lettuce water, ℥ iv; gum-syrup, ℥j½; aqueous extract of ergot, 15 grains. A tablespoonful every hour and a half.—*Ibid.*

Contagion of Typhoid Fever.—Dr. Piedvache, in the *Memoirs of the French Academy*, gives the following facts going to prove the contagious nature of this fever:

"1st. *Typhoid Fever, after attacking an individual, attacks other members of the same family in regular succession.* —From among M. Piedvache's 452 cases, in 49 there were two patients in the same house, in 30 three, in 14 four, in 13 five, in 2 six, in 1 seven, in 2 eight, and in 1 ten,—leaving but 92 (1-5), in which only one case was observed in the same house. The houses attacked nowise differed from those that escaped; and that the attacks of so many individuals in the same abodes did not result from mere local causes, is seen from the fact that such attacks were not simultaneous, three or four weeks always intervening between the first patient and his successors. The houses in which but one case occurred, were better than the others in regard to *ventilation*, as will be afterwards noticed.

2d. *An individual attacked with the fever, and transported to a family inhabiting a locality where it did not prevail, communicates it to that family.*—This is the fundamental proof relied upon by all those who have advocated the contagious nature of the disease, and especially by M. Gendrin (Journ. des Connaiss. Medic.," vols. i., and ii.), one of the ablest of these. This class of cases resembles the other in attacking several members of the same family; but the possibility of referring the propagation to any cause proper to the house which the patients inhabited, is quite cut away. The development of the disease too constantly followed the arrival of the patient to be regarded in the light of a coincidence; the

family enjoying good health prior to such arrival, and giving no signs of the disease until three or four weeks after it.

3d. *An individual attacked by typhoid transmits it to the persons who are in immediate attendance upon him, while the rest of the family does not suffer.*—Several examples of this are cited, in which the disease occurred in localities where it was not prevalent, and among the more comfortably circumstanced portions of society. In the other category, in which the disease was transmitted to *several* members of the same family, the lower orders were alone those so affected.

4th. *The attendant to whom the fever has been communicated transmits it to others.*—Among the 452 cases of typhoid fever which form the ground-work of this essay 411 either transmitted the disease, or were themselves the product of transmission—that is, 9-10 of the entire number; while those cases in which no such transmission occurred, only confirm the author's views as to the mode in which such transmission was effected in the others.

Opposed to these statements, we have the *negative* ones advanced by some of the ablest physicians attached to the Parisian hospitals; but these cannot destroy the value of *positive* facts, and only show that there are circumstances under which the phenomenon of contagion is not exhibited. Even among M. Piedvache's own cases, while of the entire 452 transmission was proved in 411, it was so in 393 of 419 country cases, and only in 15 out of 33 which occurred in the town of Dinan, and some of these 15 lived in the environs; so that in the strictness they should be considered as rural cases also. Seeing the great difference as to the manifestation of contagion which occurs in town and country, the comparative condition of the patients in each should be inquired into. As peculiarities attaching to the rustic patients, M. Piedvache mentions the number of their visitors, and the great amount of personal attendance they receive from their neighbors; the bad construction and over-crowded state of their sleeping-rooms preventing effectual ventilation; and the utter neglect of all cleanliness,—though this last is of very secondary influence as compared with defective ventilation. It is obvious that in hospitals, and in houses of the gentry, where transmission of the disease is so

rare, these conditions do not prevail. To the accidental obtaining a better ventilation, by sleeping in another room, the members of families among the poor have entirely owed their exemption when this has occurred; and to the more defective ventilation that is then procured, the author attributes the fact of more cases occurring in winter than in summer. He has never seen an example of contagion from a mere visit to a patient [the sojourn of a night in the patient's chamber seeming essential]; and priests and medical men, who are brought into very close *temporary* contact with the patient, very rarely acquire the disease.

An epidemic of typhoid does not spread from a centre, over a more or less considernble extent of country, like those of variola, scarlatina, &c. Only portions of a commune, a hamlet, or even a single house, may suffer; and once arrived in a locality, the length of its sojourn there is always remarkable. In the majority of the author's cases, the disease had reached the fourth week becoming communicated. It was so often later still, but very rarely so soon as the third week.

As several persons placed in favorable circumstances for acquiring the disease escaped doing so, some of the causes of their immunity may be adverted to. The first of these is the protection derived from a *prior attack.* M. Piedvache has never known an instance of its occurring a second time, and a patient surrounded by persons who have already had the disease does not propagate it. *Aged persons*, again, contract the disease with difficulty, though when exposed to circumstances favorable to contagion, they do not always escape. Among 138 cases, Louis found none above 39. Chomel supposed no case had been met with above 52; but since then Rayer has cited one of 56, and Prus one of 78. During the two epidemics witnessed by the author, almost all the aged persons living in houses where the disease prevailed escaped it. Still, 9 between 50 and 60, and 5 between 60 and 70, were attacked; and MM. Gendrin and Jacquet report similar examples. It was once believed that *young children* were very rarely attacked; but the researches of Rilliet and Barthez and Taupin show how erroneously. The attacks of children, in fact, often pass unperceived, and this may form some explanation of unlooked-for immunities in af-

ter life. Lastly, there are individuals whose constitution or idiosyncrasy resists the effect of contagion, under whatever circumstances they may be placed.—*Ibid.*

Pathology af Asthma.—Amongst the disturbances that may give rise to asthma, Wunderlich mentions:

1. Those occurring in the respiratory organs. Thus, congestion, extensive tubercular or cancerous deposits, emphysema, œdema, chronic bronchial catarrh, affections of the glottis, pleural adhesions, &c., have been mistaken by some writers for asthma itself, and have been regarded by others as causes of that affection. These are often complications of asthma, but we agree with Wunderlich in doubting whether they ever actually give rise to it.

2. Asthmatic attacks sometimes appear to depend on changes in the brain or spinal cord and their membranes, and on tumors pressing on the pneumogastric nerve. Alteration of structure in the brain near the origin of that nerve, chronic myelitis, ossification of the pulmonary plexus, and a tuberculous condition of the bronchial glands pressing on the pneumogastric nerve, have been described by different writers as causes of asthma. Wunderlich narrates an obstinate case of asthma recurring every week or two for several years. The patient was a man who had a small tumor under the anterior origin of the sterno-cleido-mastoid muscle, which gradually extended beneath the clavicle, and probably pressed upon the pneumogastric nerve. There was also slight emphysema, but not enough to account for the severe asthmatic attacks, which usually lasted for from three to eight days, and occurred at very short periods.

3. Affections of the heart and pericardium, and aneurism of the large thoracic vessels, have often been described as causes of asthma. Rostan even went so far as to maintain that asthma is always a symptom of an organic disease of the heart or of the large vessels. We should think that these diseases would primarily induce mechanical dyspnœa, rather than spasmodic asthma; they may, however, in a secondary manner, irritate the pneumogastric nerve, and thus actually give rise to the disease in question.

4. It has been generally assumed that deranged conditions of the stomach and intestinal canal, the liver, and the genito-urinary organs, may directly induce asthma. Whether this be the case or not, it is at all events certain that errors of diet, sexual excesses, and derangements of the digestive system, increase and aggravate the attacks of persons liable to asthma.

5. Certain conditions of the blood may cause asthmatic attacks. A superabundance of blood, or blood too rich in solid constituents, may give rise to symptoms that may be mistaken for asthma, although they are in reality symptoms pertaining to pulmonary congestion; and, on the other hand, a deficient supply of blood, or blood that is very poor in solid constituents, may give rise to symptoms that may be mistaken for asthma, although they are in reality symptoms pertaining to pulmonary congestion; and, on the other hand, a deficient supply of blood, or blood that is very poor in solid constituents, may give rise to symptoms which are, however, rather those of spasm of the glottis and of the diaphragm, than those of true asthma. The paroxysms of dyspnœa occurring in putrid and purulent poisoning of the blood, do not constitute true asthma. Wunderlich seems to doubt whether, the attacks of dyspnœa, which are so common when the kidneys are inert and the urinary constituents are retained in the blood, are really deserving of being considered asthmatic; indeed, he plainly expresses it as his opinion that in all probability the *asthma urinosum* of the older physicians and of Schonlein, is nothing more than pulmonary emphysema. As, however, he grants that lead, when introduced into the system, often produces asthma, and that other poisons not unfrequently do so, we see no reason why he should object to allow that the dyspnœa produced by the accumulation of urea in the blood is of an asthmatic nature.

Whatever may be the anatomical changes which are found in examining the bodies of persons who have suffered from asthma, the disease seems essentially to consist in a temporary and paroxysmal spasmodic constriction of the smallest bronchial canals (for the cartilaginous structure of the larger tubes will not allow of their being thus affected.)

Asthma is seldom the immediate cause of death; but

frequent attacks give rise to hypertrophy of the bronchial tubes, emphysema of the lungs, or certain forms of heart disease, which modify and aggravate the original affection, and hasten a fatal termination.

We find very few points of novelty in any of our authors in relation to treatment. Wunderlich recommends that during the paroxysm, if it comes on suddenly and with great intensity, the bowels should be emptied by a powerful injection; and he seems to think that there are many cases in which venesection would be serviceable. As, however, he adds, the arguments for and against bleeding are often nearly balanced, it is sometimes expedient to open a vein, and either to close it almost directly, or to allow the blood to flow, according to the immediate effect produced on the patient. The general testimony is, however, strongly against venesection. If the patient can drink, he should take a little warm tea containing opium, stramonium, or lobelia inflata. Strong black coffee and iced drinks have also been recommended. In those cases where the attack is more gradual, an emetic often gives great relief, and sometimes at once cuts short the paroxysm. Ipecacuanha in small doses is sometimes useful; but what Wunderlich has found most useful is a combination of tartar emetic with large doses of opium (from half a grain to one grain every hour). Stramonium smoked with an equal quantity of tobacco relieves the pains which are usually felt in the chest, but is of no other use. (This, at least, is Wunderlich's experience, who adds, that the much-praised camphor cigarettes are even less efficacious.) Constatt, as we have already had occasion to mention, enters more fully into the consideration of the individual therapeutic agents, than Wunderlich or any of our authors. We give his observations on a few of the medicines that have at different times had a high reputation in asthma:

"The Datura Stramonium has obtained a character as a specific against asthma (Krimer, Cruveilheir, Reimer, Zeigler, Ward, Marcet, Meyer, Helm, Cunningham, Hegewisch, Ferrus, Elliotson.) It is given in powder, in gradually increased doses, from two to twelve grains; in extract, from a quarter of a grain to four grains; or in tincture, from five to twenty drops. We frequently recommend the stalks and leaves to be smoked, the patient

being directed to swallow his saliva. Hegewisch orders that the number of pipes should be increased, till all the urgent thoracic symptoms have disappeared; if a relapse comes on, the patient must again begin with one or two pipes, and proceed as before. Unfortunately, asthmatic patients are often unable to inhale the smoke. The Datura fastuosa has been employed by Adams in the form of a tincture, of which thirty or forty drops were to be taken every two hours; and by Skipton in the form of decoction of its bark.

"The Lobelia inflata [Indian tobacco] was first employed by American physicians, and has recently obtained a high reputation as a remedy for asthma [Eberle, Barton, Chapman, Whitlaw, Andrews, Bidault, De Villiers, Reece, Sigmond, Elliotson, Newmann, Forbes, Morelli, Noack]. We usually give twenty or thirty drops of the tincture of the leaves at the commencement of the paroxysm, or shortly before an attack is expected; more rarely we give the powder in doses of fifteen or twenty grains. It appears to exert a specific influence on the respiratory nervous system.

"Common tobacco acts in a similar manner; it may be smoked or given in the form of tincture. . . . . The Bignonia catalpa (indigenous in Carolina) was described as a specific by Kampfer and Taubery; and has been more recently extolled by Brera, Brudent, Antomarchi, and Antonisca." (Constatt's Handbuch der medicinischen Klinik,' vol. iii. part 2, p. 450.)—*Ibid.*

American Medical Association.—We copy from the *Stethoscope* some particulars of the late meeting of this body not given in our last number. The following are the resolutions offered by Dr. Drake to which reference was made. It will be remembered that they were not adopted.

1. *Resolved,* That every report on a medical or other scientific subject shall be referred to a select committee, to be read, analyzed and reported on to the association; said select committee indicating its general character and worthiness of publication, provided the authors of every report shall have the right of appealing to the association.

2. *Resolved*, That no report shall be read before the association until it has been examined and reported on by the committee to which it may be referred; nor then but under an order of the association.

3. *Resolved*, That no report shall be published in the Transactions of the association but in virtue of its order.

4. *Resolved*, That all professional and other scientific communications made to the association, shall be referred and treated like the reports of committees.

5. *Resolved*, That the president, vice presidents and secretaries of the association shall be charged with the appointment of the aforesaid committees, being themselves eligible for such appointments.

6. *Resolved*, That the authors of all reports and papers aforesaid shall have the privilege of reading and explaining the same before the committees.

The report of the committee on the Medical Botany of the U. S. for 1850–'51, from Dr. A. Clapp, chairman, was presented and referred to the committee of publication.

THURSDAY, May 6, 1852.

The association was called to order at 9½ o'clock—Dr. Wellford, president, in the chair.

The minutes were read, amended and approved.

Dr. Green, of N. Y., offered the following resolutions, which were adopted:

1. *Resolved*, That at all future meetings of this association all reports of committees and all contributions on scientific subjects occupying more than ten pages of quarto post manuscript, be accompanied each by an abstract or synopsis embracing the principal points of such report or paper, which abstract or synopsis may be read before the association.

2. *Resolved*, That the above resolution be transmitted by the secretary to the chairman of each scientific committee.

Dr. Sutton, of Ky., moved that a committee of three be appointed, whose duty it shall be to inquire whether any, and if any, what action this association shall take in reference to requesting the Congress of the United States to have a large edition of the medical statistics, furnished by the census lately taken, published in a separate form for distribution among the medical profession of the United States, and to report to-day.

The chair announced the committee, to consist of Drs. Simons, of S. C., Boyle, of D. C., and Sumner, of Conn.

The report of the committee appointed to consider the various propositions which had been made, suggesting amendments to the constitution, being called for, the chairman, Dr. F. Campbell Stewart, of N. Y., read a report and resolutions, which Dr. Hays, of Pa., moved to refer to the committee of publication, with instructions to print.

Drs. J. K. Mitchell, of Pa., and W. Hooker, of Conn., discussed the merits of the report, when Dr. Lopez, of Alabama, raised a question of order as to the propriety of a discussion of the merits of the proposition.

The chair decided that the discussion was in order at this stage of the proceeding.

Dr. Lopez, of Ala., appealed from that decision, which appeal was not sustained.

The discussion was then continued at great length by many members.

During the discussion, the following replies were elicited from several gentlemen, by questions propounded by Dr. Watson, of New York:

From Dr. Horner, University of Pa.—The shortest term of medical study in the University required for the doctorate was three years, but that under some few and rare circumstances, a deviation had been permitted as an exception.

Drs. Davis and Rogers, of Virginia University, stated that their laws required no specified time: nine months, and eighteen years of age even, were sufficient, but that two years were generally devoted to the study of medicine by their graduates. They explained the course of instruction at the University at length.

Dr. Huston, of Jefferson Medical College, Philadelphia, said that three full years were required, but that occasions demanded sometimes a departure from the stringent rule.

Dr. Frost, from South Carolina, offered some interesting observations upon the much abused subject of medical education, and insisted that the profession had not retrograded. That there had been a steady and gradual improvement in our medical colleges generally, and brought to the notice of the association the attention

which was observed in preparatory education in the medical college of South Carolina, which was highly creditable to the same. His remarks were listened to with attention, and brought forth observations of a like character from other members present.

The report and proposed amendments were received, after having been amended so as to read as follows: (We omit the report.)

ARTICLE I.—*Title of the Association.*

This institution shall be known and distinguished by the name and title of "The American Medical Association." It shall be composed of all the members of the medical profession of the United States of good standing, who acknowledge fealty to and adopt the code of ethics adopted by the association; and its business shall be conducted by their delegates or representatives, who shall be appointed annually in the manner prescribed in this constitution.

Strike out the whole of Article II, referring to "Members," and insert the following:

ARTICLE II.—*Of Delegates.*

Sec. 1. The delegates to the meetings of the association shall collectively represent and have cognizance of the common interests of the medical profession in every part of the United States, and shall hold their appointment from county, State and regularly chartered medical societies; from chartered medical colleges, hospitals and permanent voluntary medical associations in good standing with the profession. Delegates may also be received from the medical staffs of the United States army and navy.

Sec. 2. Each delegate shall hold his appointment for one year and until another is appointed to succeed him, and he shall be entitled to participate in all the business affairs of the association.

Sec. 3. The county, district, chartered and voluntary medical societies shall have the privilege of sending to the association one delegate for every ten of its resident members, and one more for every additional fraction of more than one half of this number.

Sec. 4. Every State society shall have the privilege of sending four delegates; and in those States in which county and district societies are not generally organized, in lieu of the privilege of sending four delegates, it shall

be entitled to send one delegate for every ten of its regular members, and one more for every additional fraction of more than one half of this number.

Sec. 5. No medical society shall have the privilege of representation which does not require of its members an observance of the code of ethics of this association.

Sec. 6. The faculty of every chartered medical college acknowledging its fealty to the code of ethics of this association, shall have the privilege of sending one delegate to represent it in the association: *Provided*, That the said faculty shall comprise six professors, and give one course of instruction annually of not less than sixteen weeks on Anatomy, Materia Medica, Theory and Practice of Medicine, Theory and Practice of Surgery, Midwifery and Chemistry: *And provided also*, That the said faculty requires of its candidates for graduation—1st. That they shall be twenty-one of age; 2d. That they shall have studied three entire years, two of which must have been with some respectable practitioner; 3d. That they shall have attended two full courses of lectures, [not however to be embraced in the same year,] and one of which must have been in the institution granting the diploma, and also where students are required to continue their attendance on the lectures to the close of the session; and 4th. That they shall show by examination that they are qualified to practice medicine.

Sec. 7. The medical faculty of the University of Virginia shall be entitled to representation in the association, notwithstanding that it has not six professors, and that it does not require three years of study from its pupils, but only so long as the present peculiar system of instruction and examination practised by that institution shall continue in force.

Sec. 8. All hospitals, the medical officers of which are in good standing with the profession, and which have accommodation for one hundred patients, shall be entitled to send one delegate to the association.

Sec. 9. Delegates representing the medical staffs of the United States army and navy shall be appointed by the chiefs of the army and navy medical bureaux. The number of delegates so appointed shall be four from the army medical officers and an equal number from the navy medical officers.

Sec. 10. No delegate shall be registered on the books of the association as representing more than one constituency.

Sec. 11. Every delegate elect, prior to the permanent organization of the annual meeting, and before voting on any question after the meeting has been organized, shall sign the constitution and inscribe his name and address in full, with the title of the institution which he represents.

FRIDAY, May 7, 1852.

The association was called to order by the president at 4½ o'clock, P. M.

The minutes of yesterday's session were read, amended and approved.

Dr. Atlee, of Pennsylvania, offered the following preamble and resolution, which was unanimously adopted:

Whereas it is the duty of patriotism to do homage to those who have been benefactors to their country; and whereas the medical profession in the United States, heretofore not wanting in patriotic feeling or action, desire to co-operate with the other public bodies and institutions of the country in rendering their profound reverence to the memory of him who was "first in peace, first in war, and first in the hearts of his countrymen:"

Be it therefore resolved, That a committee of five be appointed, whose duty it shall be to solicit subscriptions from members of the American Medical Association, for the purpose of procuring a suitable stone with an appropriate inscription, for insertion in the name of this association, into the national monument to the memory of Washington now in progress of erection at Washington city.

Dr. Wood, of Pa., then moved that instead of awarding five prizes of $50 each, annually, the association hereafter grant two prizes of $100 each, for the two best essays. Carried.

On the Distribution of the Crinoidea in the Western States. By Prof. L. P. Yandell.—Having been engaged for several years past with my friend, Dr. B. F. Shumard, in collecting materials for a monograph on the crinoidea of the Western States, I propose to submit a few details respect-

ing the distribution of the remains of this interesting class of animals through the palæozoic rocks, of this portion of the Mississippi valley. The richness of this quarter of our country in these fossils has already been made known to the scientific world by the labors of the late lamented Dr. Troost, whose monograph on the crinoidea of Tennessee is in course of publication under the auspices of the Smithsonian Institution. The list of the great naturalist of Tennessee includes about thirty genera and a hundred species, most of which, when he wrote, were undescribed.

The collection of Dr. Shumard and myself includes specimens from Ohio, Indiana, Illinois, Iowa, Missouri, Tennessee, Kentucky, and Alabama, extending from the lower Silurian rocks to the beginning of the coal series, and amounts to about twice the number of species described by Dr. Troost. In the present state of our knowledge we would not venture to pronounce positively on the number, but we believe it will be found rather to exceed than fall short of thirty-five genera and two hundred species, of which a few are described by Prof. Hall, in the Palæontology of New York, some have been published by Drs. Owen and Shumard, in the Journal of the Academy of Natural Sciences of Philadelphia, and many are embraced in the forthcoming work of Dr. Troost; but a great part are new.

One of the first facts which arrested our attention in regard to our crinoids, is the limited geographical range of all but a few species. While we met with the same brachiopods and corals over wide areas, we soon had occasion to remark that encrinites were limited to a few narrow localities. *Caryocrinus ornatus* is perhaps the most widely distributed of our species. It is found in Louisville, Ky., and in Decatur county, Tenn., as well as at Lockport, N. Y., where it was first discovered. One of Dr. Troost's new species is also widely diffused—the *Agaricocrinus tuberosus*—having been found at White's Creek Springs, near Nashville, Tenn., in the vicinity of Scottsville, Ky., and at Warsaw, Ill.; and the same is true of *Olivanites verneuili*, Troost (formerly *Pentremites verneuili*), which occurs abundantly in the upper strata of the Falls of the Ohio, and presents itself in the States of Ohio and Indiana wherever those strata are exposed. A

few other species occur at various points, but much the greater number appear to have been restricted to a single spot.

With so circumscribed a range, it will not be thought singular that nearly all the species in our cabinets are peculiar to the Mississippi valley. I am aware how unsafe it is to decide hastily as to the identity or diversity of species bearing a close resemblance to each other, but after the full investigation I have had time to give the subject, my impression is, that not a single one yet found in the Western States is common to Europe and this country. Drs. Owen and Shumard incline to the belief that *Platycrinus granulatus*, and *Poteriocrinus plicatus* belong to our formations. It may be so, but if the European and American species are identical they form solitary exceptions amid a great multitude of forms. We have traced analogy everywhere, but examples of identity are still doubtful. The analogy between our species and those of Europe occurring in equivalent systems is, indeed, one of the most striking and interesting facts discovered in the progress of our researches. The *Cystidea* of Russia, Sweden, and England have their analogues in the Silurian rocks of Kentucky and Tennessee. Troost's *Echinocrinus fenestratus* answers to species of Echino-encrinus which occur in Russia; and we have lately discovered in the neighborhood of Louisville, and in Decatur co., Tenn., specimens of what Prof. Hall pronounces a *Spheronites*, corresponding to the same form which has been found in the Silurian strata of England. *Caryocrinus*, which is peculiarly American, is represented by *Hemicosmites* at St. Petersburg. The *Agaricocrinus tuberosus* of Troost has a striking resemblance to *Actinocrinus amphora*, found in equivalent formations in Ireland. The genera *Platycrinus*, *Actinocrinus*, and *Eucalyptocrinus* occur in the rocks of our country which are equivalent to those affording them in Europe, and contain species which it is difficult to distinguish from their European analogues—so difficult, indeed, that several have been taken for the same.

We have had many opportunities to remark the exceedingly limited vertical range of our crinoidea, as other observers had done before, and are prepared to assert with great confidence, that not one of our species occurs in more than one formation. For the most part, each is

contained in a thin and well-defined stratum, beyond the borders of which it is seldom to be traced. Not a single species is common to the lower and upper Silurian systems, or to the Silurian and Devonian, or to the Devonian and Carboniferous, much less to the three systems; and the same, with slight qualifications, might be said of genera, as will presently appear. No fossils, it is believed, will be found to be more characteristic of their containing strata, or to mark more precisely the limits of succeeding systems, than the crinoidea.

We have met with crinoidal remains at a point lower in the geological scale than they were ever before discovered. Dr. Shumard first saw their broken columns in Wisconsin and Minnesota, several hundred feet below the magnesian limestone, in strata corresponding to the Potsdam sandstone of New York—showing that this curious race of beings were among the earliest tenants of the deep. They appeared with the primeval brachiopods and crustaceans, and have maintained their existence through every successive epoch, but in varying proportions, and in singularly diversified forms. In the oldest Palæozoic rocks we detect only, scattered here and there, the fragments of their stems and bodies. In the Trenton group of New York, and in its equivalent rocks in the West, we find a few genera and species, in a good state of preservation. In the Niagara group, at the middle of the Silurian system, we discover a sudden and great expansion of the race, which soon after seems to have begun to decline, and to have been less abundant throughout the Devonian era. But in the age which followed, they attained a great development; the Carboniferous era was the age of the crinoids. Thus, in our collections, we have from the lower Silurian system six genera and six species; and from the upper beds of that system, chiefly the Niagara group, we have ten genera and forty species. From the Devonian rocks, we have ten genera and seventeen species; while from the Carboniferous system we have twelve genera and one hundred and fifty species.

I do not suppose that these figures represent accurately the ratio of the species in the several systems, for our opportunities for collecting from all have not been equally good; but I feel assured that they will be found to be an approximation to the truth. At Louisville, the Devonian

rocks are well displayed; but we have found them less rich in these fossils than the strata of an earlier age; and from one or two localities in the Carboniferous formations, we have obtained a greater number of species than this system has hitherto afforded. The fact seems to be a general one, that the crinoidea experienced a remarkable increase about the middle of the Silurian epoch—that they were stationary, if they did not decline, during the Devonian era, and reached their utmost expansion in the ages that succeeded. It has been remarked by Professor Hall, as true of the formations of New York; and the same has been the experience of European geologists.

With the succession of formations, we have a remarkable shifting of genera, and change of form, and external ornament. *Glyptocrinus*, *Schizocrinus*, *Heterocrinus* and *Agelacrinu* cease with the older Silurian strata, and are succeeded by *Haplocrinus*, *Eucalyptocrinus*, and by *Caryocrinus*, *Balanocrinus*, *Cupellæcrinus*, and many undetermined genera peculiar to our continent. These, in turn, are left behind when we ascend to the Devonian series, in which we find other forms and new genera. Here we meet with a crinoid with four bulky arms, destitute of a stem—a free-moving encrinite, showing thus early an approach in this family towards the starfishes, placed just above them in the animal scale. At this point, too, occurs our first *Actinocrinus*, which, like the forms abounding in the preceding series, is remarkable for the beauty of its external ornament. And here we also encounter a new crinoidal type—the *Pentremites*—already prefigured in *Eucalyptocrinus* of the middle Silurian period. Dr. Troost described as *Pentremites Reinwardti*, a form which he was the first to discover in Decatur county, Tenn., and which, from its associated fossils, has been generally assigned to the Silurian age. Strictly speaking, *Pentremites Reinwardti*, though bearing the closest analogy to that group, is not a pentremite, but must be placed in a new genus along with some other organisms found in rocks of a similar age. From the fact that it is found in Tennessee accompanied by *Calceola*, a Devonian fossil, and that we also find it at the Falls of the Ohio, in strata unquestionably Devonian, I do not hesitate to refer it to this system. Along with it, at the latter locality, two or three species occur closely related to it, together with *Olivanites Ver-*

neuili Troost, and one or two species of the same genus.

Just above the beds containing these forms, the limestone series cease, and the black slate is superimposed. With the incoming of this formation, we lose all vestiges of the crinoids, until we reach an intercalated stratum of limestone near its summit, where encrinital remains again become abundant. This stratum of encrinital limestone is regarded as the base of the Carboniferous system, containing as it does among its imbedded fossils, *Orthis Michelini*, *O. crenistriæ*, *Terebratula Roissyi*, *Spirifer striatus*, *Productus punctatus*, *P. semireticulatus*, *Phillipsia*, *Griffithidis*, and encrinites of forms known only to that system in Europe.

The aspect of the crinoids of this period is eminently distinctive—in some respects denoting inferiority to the older forms, in others indicating advancement. The exquisitely ornamented surface has, for the most part, disappeared, and a plain exterior succeeded; but with it we have tentaculated fingers, and a generally more complicated organization. In this age *Actinocrinus* attained its full development, and the species of *Cyathocrinus*, if they did not appear here for the first time, only became abundant at this period. *Platycrinus*, incorrectly placed in the older series, had its beginning with this formation, and was accompanied by *Dichocrinus* and *Synbathocrinus*, by Troost's *Conocrinus*, *Agaricocrinus*, *Catillocrinus*, and by many other new, undetermined genera. The graceful *Agassizocrinus* of Troost, a stemless, free-swimming form, like the asterias, was rife in this age, and had for its companions *Melonites* and the true *Pentremites*, which latter must have swarmed in the seas that gave origin to our mountain limestone. *Pentremites florealis* occurs near the base of this formation, and is found in all its divisions up to the topmost strata. With the pentremites, we find still another novel form, intermediate between them and the new genus *Melonites*—the *Granatocrinus cidariformis* of Troost; and here, too, we meet with *Cidaris* and other echinoderms.

This succession of genera is a striking feature in the history of the crinoidea, and their extinction at various periods, affords a problem of great interest to the zoölogist. Why they should, one after another, have perished from the early seas in which they lived, to be succeeded

by other and still distinct forms, is a mystery which must perhaps in most cases remain forever unexplained; but in some instances a satisfactory solution seems to be attainable. Sir Charles Lyell, in his Manual of Elementary Geology, describes the "Bradford apiocrinites" as having grown in an oolitic limestone, upon which their stony roots now form a continuous pavement; a layer of clay rests upon it, containing the bodies and stems of innumerable encrinites. "In this case," he remarks, "it appears that the 'Great Oolite' had supported, for a time, a thick submarine forest of these beautiful zoophytes, until the clear and still water was invaded by a current charged with mud, which threw down the stone-lilies, and broke most of their stems short off near the point of their attachment." At several localities I have observed traces of a similar succession of events. The Buttonmould Knob, in the vicinity of Louisville, affords a striking example. A thin bed of limestone, deposited near the top of the Black Slate, is made up almost exclusively of the exuviæ of encrinites, showing that these animals abounded in clear water charged with carbonate of lime. The formation changes, and the crinoids disappear, having been entombed in the argillaceous deposit in which their remains are imbedded. In places where the clay is unmixed with iron, the fossils are in an excellent state of preservation; where iron mingles with it, they are generally fragmentary and imperfect.

We have satisfied ourselves of the carnivorous habits of the crinoidea. In one of our earliest excursions, we discovered a curious encrinite with an acroculia between its fingers, as if it had been in the act of devouring it when it perished. At Cincinnati we have been shown several specimens, in which a trochus is thus entangled in the rays of *Glyptocrinus decadactylus*, and we have lately received from our friend, Mr. Worthen, of Warsaw, Ill., a platycrinus, and more than one species of actinocrinus, with an acroculia in a similar position. The gasteropods seem to have been their favorite article of food.

The prevalence of the crinoids in this country, as compared with Europe, is a fact that will strike every observer. Dr. Troost supposed that he had collected together the remains of a greater number than were at that time known to European palæontologists, and the list known

to him has been already more than doubled. These organisms are to our continent what the crustaceans are to Europe—the ocean-lilies swarmed along the shores of our early seas, as the trilobites did in the ocean which has given place to dry land in the old world. Entering at the same time upon the stage of existence, at the earliest dawn of creation, the trilobites reached their meridian in the first Silurian epoch, and died out at the beginning of the Coal period. The crinoids only attained their full development in the Carboniferous era, and, extending through the Mesozoic period, have still their representatives among existing races.—*Proceedings of the American Association.*

Quinine in Typhus and Typhoid Fevers.—On this subject, Professor Bennett read a paper to the Medico-Chirurgical Society, on Wednesday evening. It appeared, that about the month of November last, Professors Bennett and Christison had received communications from Dr. Dundas, of Liverpool, mentioning the beneficial results he had found to follow the exhibition of quinine in the continued fevers of warm climates. So strong were the representations of Dr. Dundas, that Professor Bennett, whose period of clinical duty in the Infirmary had at that time just commenced, determined to give the remedy a fair trial. The doses Dr. Dundas recommended were ten grains every second hour. Dr. Bennett had given quinine in seven out of fourteen cases of typhus or typhoid fever admitted into the Royal Infirmary during the period from November to the end of February. In all seven the physiological effects of the remedy had been produced. In most of the cases, the seventh was the day of the disease on which the treatment of quinine was begun, in some it was later; one patient, however, had it on the sixth day, and none later than the tenth. In one case, 205 grains of quinine had been taken in eighteen doses. In five others about 80 grains had been taken. Dr. Bennett had in none of these instances found the quinine to cut short the disease, or in any way favorably to influence its progress. In one case convalescence had been delayed till the forty-second day. At the same time, Dr. Bennett thought it proper, in bringing the sub-

ject before the Society, to mention, that the experience of some other physicians had been more favorable. Dr. Graves, of Dublin, for example, had written to Dr. Dundas to that effect, and so had Dr. Kelly, of Drogheda, who had treated eight cases with the happiest results. Professor Christison's experience had as yet been limited to one case, that of a girl who had contracted typhoid fever in the hospital when recovering from another disease. In it the employment of the remedy had certainly produced no favorable effect—it was now the forty-second day, and no marked convalescence had commenced. He regarded Dr. Bennett's experiments as of additional value from having been made *coram publico*, in the presence of a large number of intelligent students, many of whom had been from time to time examined on the very cases themselves. Important as the inquiry was, he much feared that the discovery of quinine as an antidote in continued fever would prove of but little avail, unless the additional discovery of new cinchona forests was also made.

Dr. William Robertson had also administered the quinine in some cases, in none with benefit. He had in two cases observed very alarming symptoms to follow its exhibition, a state of almost complete coma having been induced.—*Medical Times.*

THE WESTERN JOURNAL.

THIRD SERIES.—VOL. X.—NO. I.

LOUISVILLE, JULY 1, 1852.

CONTAGIOUSNESS OF TYPHOID FEVER.

Dr. A. B. Chambers, of Warsaw, Kentucky, has communicated to us the following facts bearing upon the *contagiousness of Typhoid Fever*:

"I have a friend residing on a wide bottom of Eagle creek, in this county. He has lived there for ten or fifteen years, his family enjoying excellent health. The latter part of last summer he sent out one of his daughters to school in Louisville, where she remained about two months, when becoming indisposed she was brought home. Her sickness proved to be typhoid fever in a very distinct form. Her recovery, which took took place in from three to five weeks—not now distinctly remembered—was followed by an attack of her brother, and subsequently of her parents, her grandfather, who resided in the family, and all her other brothers and sisters (six in number) except one, and some seven or eight negroes. When all the other members of the white family had become sick, the little daughter, who had as yet escaped, was sent to the house of the gentleman's mother, in the hope that a change of locality and removal from the contaminated atmosphere of home might secure her from an attack. She remained there for three weeks, to all appearance in

perfect health, with the exception of a slight inflammation of one of her eyes, which yielded to simple treatment. At the end of this time she was taken down with the same fever, in a well marked form, and was sick for two months. In the latter part of the winter, a young lady, a relative of the family, residing at Midway, in Woodford county, hearing of the affliction of her friends, came down to assist in nursing them. She remained two weeks in constant attendance on several of the family who were then slowly recovering, and then paid a visit to the little daughter who was lying sick. At this house she was attacked in a day or two, and had a hard spell of fever, lasting some six weeks, but is now again restored to health. In the early part of her sickness, a negro girl belonging to another member of the family, was employed to assist in nursing, and particularly to wash the clothes of the sick persons. Some ten or fifteen days after returning home, she was attacked with the disease in a mild form, so mild that it did not confine her constantly to bed, but still with the characteristic symptoms distinctly marked—such as muscular debility, continued fever, rapid pulse, abdominal tenderness, liquid stools, &c. She is now well; but a negro man who slept in the same house with her, has been attacked with the same symptoms in a severe form. In addition to these, a brother of the gentleman, who spent some days in his family during their sickness, on returning to his home, was sick for some two weeks, though of the precise character of his symptoms I am not informed, as I did not see him till he was much improved. He thinks, however, that his disease was similar in character though not so severe as that he saw in his brother's family.

Up to this time there had been but little of this fever in our county, in comparison with many in the interior of the state; and during the period mentioned there was not more among the other inhabitants than usual.

Until those cases came under my observation I had not met with anything to authorise the belief that typhoid fever is contagious, nor did I believe it; but I confess that if my opinion has not undergone an entire change, my faith in its non-contagious character is greatly shaken.

Some of these cases presented the disease in a grave form; in most of them it was mild; but in all the symptoms were highly characteristic."

Warsaw, May 13th, 1852.

THE SPIRITUAL RAPPERS ONCE MORE.

Our readers will remember the exposure made of the "Spiritual Rappers," by Dr. Lee, Dr. Flint, and others, at Buffalo, a few months

since. Notwithstanding that the pretensions of these young ladies were so effectually exposed on that occasion, they are still perambulating the country, imposing upon the credulous, and turning the heads of a few weak people. They paid a visit to Kentucky, a short time since, and were in Lexington, where they were met by our friend, Dr. Darby. We have received from the doctor a long and amusing account of his interview with the *Misses Fox*, the sum of which is, that when they found he had come to investigate rather than to stare, they refused to perform. We would give the whole of his interesting letter if we did not deem the humbug unworthy of any further notice. Suffice it to say, the Misses Fox made a very short stay in Lexington. They felt that their tricks had been exposed, for, Dr. Darby adds, "they would not appear the day after he saw them although they remained in the city till the afternoon train of cars left for Louisville." We learn from Dr. D., that more than one physician had marvelous stories to tell of the fair jugglers, after an interview with them, and no doubt that wherever the "Rappers" go they will find, even among the learned, good subjects to help on with the imposture. Nevertheless, we think it must now soon come to an end.

ASSOCIATION OF BOTANISTS.

We learn from the Cincinnati papers, that an Association of Systematic Botanists, having for its object the study and advancement of Systematic or Scientific Botany, has been recently organized in this city; President, Dr. John A. Warder; Secretary, James W. Ward, Esq. The objects of the Society are eminently practical, embracing the classification, nomenclature and normal habits of plants and the definition of their general characters, together with the history and description of the insects that feed on and destroy them. Persons willing to co-operate with the Society in any of its objects, or desiring information in regard to specimens they may have obtained, will receive attention by communicating with Mr. Ward, the General Secretary.

In this connection we beg leave to call the attention of our readers to the *Natural History Society of Louisville*, and would say to any of them who may have specimens of minerals, fossils, or animals respecting which they are desirous of obtaining information, their wishes will be attended to if they will forward them to Dr. Clapp, the President, or to Dr. L. P. Yandell, the Secretary, at the University of Louisville.

HISTORY OF SURGERY IN KENTUCKY.

We invite the attention of our Kentucky readers to the subjoined circular. And are there not among the many physicians who have gone from Kentucky, some who could communicate valuable facts touching the surgical history of their native State? All who are in possession of information on this subject will, no doubt, take pleasure in contributing to make the proposed history complete.

To the Physicians of Kentucky.—The undersigned, chairman of the committee on surgery, of the Kentucky Medical Society, begs leave, in compliance with the duties assigned to him, to call the attention of his professional brethren, in all parts of the State, to the subject, and to request them to send to him, on or before the 15th of August next, all such facts and observations within their knowledge as may be calculated to illustrate the history and practice of surgery, from the earliest settlement of the country to the present moment. It is the sincere desire of the undersigned not to overlook or pass unnoticed the claims of any one, and he therefore earnestly invites the cooperation of all.

Louisville, June 19, 1852. S. D. GROSS, M. D.

UNIVERSITY OF LOUISVILLE.

Professor Drake's devotion to Cincinnati, and to the school of his earlier days, has again prevailed with him to resign his chair in the University of Louisville, and return to the Medical College of Ohio, and Professor Cobb has followed his example. The paramount consideration with the latter gentleman for giving up a place which he had so long and so creditably held, was the opportunity afforded by the change of securing an appointment—that of Demonstrator of Anatomy—for his promising son, Dr. William H. Cobb. The separation of those gentlemen from their old associates is without any diminution of that mutual esteem and good will, which has subsisted between them during all their professional intercourse.

The Board of Trustees have filled the vacancies by the appointment of Professor Austin Flint, of the University of Buffalo, to the chair of *Theory and Practice*, and of Professor Benjamin R. Palmer, to the chair of *Anatomy*. Professor Palmer is connected with the University of Buffalo, as Professor of Anatomy, and has for a number of years held the same chair in the Medical College of Vermont, at Woodstock.

We have delayed the issue of our present number a few days for the

purpose of being able to make this announcement, and have now space for little more than to congratulate the alumni and friends of the University upon the very satisfactory manner in which the Board have supplied the places of Profs. Drake and Cobb. Like their predecessors, Professor Flint and Professor Palmer are men of a national reputation, well tried and eminently successful as teachers. As a lecturer, those who have been associated with him speak of Professor Palmer in the highest terms. His lectures and demonstrations are described as being clear, accurate, thorough, and his manner one to awaken enthusiasm in the mind of the student, secure his unremitted attention, and enforce the acquisition of anatomical knowledge. Professor Flint at one time held a chair in the Medical College of Chicago, and has been for a number of years Professor of the Theory and Practice of Medicine in the University of Buffalo. His experience both in private and hospital practice has been ample, and he is known everywhere in our country as a writer on practical medicine. His essay "*On Variations of Pitch in Percussion and Respiratory Sounds,*" as has already been noticed in this journal, received the prize at the late meeting of the American Medical Association, and was declared by the committee to have enlarged the bounds of medical science. He is a polished, vigorous, and original writer, and a clear, instructive, and eloquent lecturer. The accession of such men to the school is at once the best evidence of its high character abroad, and the surest guaranty of its future success.

These gentlemen have accepted.

UNIVERSITY OF NASHVILLE.

We have before us the Second Annual Announcement of the Medical Department of the University of Nashville, from which it appears that the class in that institution last winter, numbered one hundred and twenty-one. The number of graduates was thirty-three No medical school in our country ever had so prosperous a beginning.

THE

WESTERN JOURNAL

OF

MEDICINE AND SURGERY.

AUGUST, 1852.

ART. I.—*A Case of Spinal Irritation, attended with loss of Vision; Recovery.* By CHARLES FISHBACK, M. D., of Shelbyville, Ind.

John Smith, æt. 30, temperament sanguineo-nervous, the latter predominating decidedly; florid complexion, light hair, blue eyes, brain large anteriorly and superiorly; health feeble in childhood, but generally good since puberty. At about five years of age was blind for several weeks, cause unknown, eyes were red, but not painful. The physician who was called in, said he could save the right eye, but not the left, by putting in it a powder. This caused great swelling, threatening the bursting of the globe, and violent pain. The sight of the left eye was restored, but the right was of no use until the winter of 1850 and '51, after the loss of vision of the left eye, when the function of the right eye gradually and steadily improved, until in July, 1851, the pain which had been

previously confined to the left side invaded the right, and then impairment of vision of right eye occurred, and progressed to complete loss early in November, 1851. He has a sister older than himself, who was blind for some time during her youth, but has not been since.

Mr. S. is a farmer by occupation; has been married four years, and has two children. In September, 1850, was attacked with pain of the left side of his head and face, extending exactly to the mesial line anteriorly and posteriorly, and embracing all of that side of the head and face excepting the globe of the eye. About the middle of November, 1850, having taken severe cold from getting wet, violent pain appeared in the globe of the left eye, and in a few days, vision of that eye became impaired, the globe assuming a more prominent appearance, and becoming, to his friends, larger than formerly but without redness; vision entirely lost in about two months. About the time of taking cold in November, Dr. McGeaughy was called in. Prescribed an active cathartic, to be followed by quinia and Ferri carb. in large doses, without sensible diminution of pain, and causing great irritability of stomach, attended with distressing flatulency and rejection of every article of food and drink. The quinia and iron were continued at intervals until May. Large portions of opium and morphia were given during this period, to allay the almost insupportable pain, inducing the ordinary unpleasant effects of opium in large and successive doses, in an aggravated degree. Dr. M. also gave guiac for rheumatism, which he said was a complication. There was pain of the shoulders and upper third or more of spinal column.

In January, 1851, there was a remission of the pain for a short time. In April, there was entire absence of pain for two or three weeks, and he went to work. During the latter part of winter and early in the spring, resort was had to electro-magnetism, sometimes with relief to pain, but not at others.

Towards the close of May, 1851, he visited Cincinnati, and consulted Prof. Mussey, whose written opinion and prescription are as follows, viz: "There is some reason to hope for relief from a suitable diet, with some medicine. Never let the bowels become costive. If there be a tendency that way, take at bed-time of rhubarb and charcoal, each 10 grs., and ginger 5 grs., mixed together; or take occasionally at bed-time, one of the following pills, viz: Ext. colocynth comp. ℈iiss, sapo. castil. ℈ss, oil Croton, gtt., j. m. ft. pills, 12. One hour after eating, take, in a tablespoonful of rain water, four drops of the solut. Fowleri mineralis; continue it for five or six weeks, unless the vision is very much improved in that time, or some of the specific effects of the article should manifest themselves. In either case, its use may be suspended. Use no tobacco in any form, nor strong drink, nor coffee. Avoid the eating of meat. Perhaps you may take a boiled egg once a day, if it be found to agree with the stomach. Drink weak black tea, or water. Eat no bread that is less than one day old. Never eat anything between meals."

There had been a tendency to constipation all along. No sensible effect was produced by the solution Fowleri mineralis, though taken according to directions. Only one or two of the pills was ever taken. There was comparative comfort from middle of May to middle of July, at which time pain attacked the right side of his head and the right eye. He now came under the care of Dr. Wolf, who removed six fangs, and ordered solution sulphate of magnesia to keep the bowels open, and pills containing a mercurial, which soon produced slight ptyalism, without any improvement, but on the contrary great prostration, so as to require aid in changing his place from bed to chair. He also gave, night and morning, a pill containing strychnine. He also ordered a resumption of the quinia and iron. No benefit followed, but general and complete prostration.

He came under my notice October, 1851, very weak, much reduced in flesh, pulse feeble, and irritable, and a little over 100 beats a minute. Tongue furred and deeply fissured in every direction over its entire surface. Bowels torpid, gastric flatulency great, and frequently joined with acidity. Appetite poor and irregular. No enlargement of abdominal viscera perceptible, nor tenderness, except of epigastrium, which is exceedingly tender; urine high colored, and variable in quantity. Excessive tenderness over the entire spinal column, a little greater in the upper two-thirds of the dorsal region than elsewhere; complains much of pain and weakness of the back. Pain of the right side of the head constant and severe, but variable in degree, being greatest when flatulency and acidity of the stomach prevailed. Has been troubled with almost icy coldness of feet since about the time that stomach became irritable, or nearly a year, and is most felt when pain of head is severest. For sometime past, cannot sleep more than two or three hours in twenty-four, and often less; sometimes none at all. Sense of fatigue throughout the muscular system, but no pain or tenderness. Pupils dilated almost to their full extent, but contracting considerably in a strong light. Cornea clear; conjunctival color not perceptibly altered. A very few tortuous vessels over the sclerotic portion of the globe. Neither the circumcorneal zone, nor the straight, and fine vessels indicative of sclerotitis visible with a good pocket magnifier. Globes prominent, with fullness of palpebræ, upper and lower; vision of the left eye entirely gone. Can see a little with right eye. Coldness of extremities, superior and inferior, general, intense, and almost constant.

Regarding the state of vision as dependent mainly upon disease of the optic nerve, or of that part of the brain from which it arises, and *this partially*, upon the state of the constitution, I directed treatment to the latter, First:

Kept the bowels in a soluble state, with pill colocynth, compos., of the United States Dispensatory, modified by the addition of gamboge and rhubarb. Quieted his nervous system and stomach, by scarified cups over spine and stomach, followed by emplast pix. Burgund.; hot pediluvia with mustard; nitro-muriatic acid to entire cutaneous surface, and iij grain pills of ext. hyosciamus, repeated every hour or two, until the desired effect is obtained. Endeavored to correct acidity of stomach with Bismuthis subnitrat., which was only partially successful, neutralizing, but not preventing acidity. During the month of October, his improvement was steady, and much more rapid than I had dared to hope for. Though not perfectly free from pain of the head, he was comparatively so. Sleeps sufficiently, and it is refreshing, and much of the time without the anodyne. Pulse became slower, about 80 per minute, stronger and calmer. Bowels soluble, and daily moved, without medicine after the first week. Tenderness of spine and epigastrium scarcely perceptible. Strength much increased; spirits revived and cheerful. Vision of right eye much improved, without sensible change in the appearance of the eye.

October 24th. Introduced a seton in the nape, and kept it running for about six weeks. A week or ten days after its introduction, he contracted a severe cold, from exposure during the greater part of a night, caring for a sick horse. An increase of the general malaise, and of the head in particular, followed. Ung. veratria to forehead, mastoids and nape; Ung. tart. antimony to epigastric and lumbar spinal regions, after scarified cups and pills argent. oxidi, grs. ij, ter die, November 12th, and subsequently, with benefit.

November 18th. Having suddenly risen from his seat, and stepped to the door, he fell to the floor, and was insensible and apparently breathless for some minutes; face pale. About an hour afterwards, he commenced

talking, as if he had been asleep, without recollection of anything after reaching the door. This I suppose to have been syncope. For a week afterwards the pain, coldness of extremities, and restlessness, were greatly increased, with loss of appetite. There were also occasional flushings of the face, with a sense of fullness about the head. With the renewal of pain, came diminution of vision, until in a few days, it was entirely lost.

He was at home nine miles from town on the 18th, and until the 28th. I visited him on the 22d, and found him suffering beyond expression. Ordered, ℞. syrup sarzae comp. (U. S. Dispensatory,) ℥j, potassii iodidi, grs. v, ter die. Caused slight nausea, but was continued. Pills ext. hyosciamus, and frictions to cutaneous surface, with hot pediluvia. He got better and came to town on the 28th. The next day, I applied one leech to the scalp near the anterior and inferior portion of the right parietal bone, and another over the occipital bone, about an inch above and behind the mastoid, of same side; at both of which points there was considerable tenderness. Ordered, ℞. magnesia sulphatis, ℥i, ant. tart. grs. i, Vin. colchici f. ℥ss; aquae ℥iv, M. S. a table spoonful sufficiently often to insure two or three dejections daily.

December 3d. Two leeches to anterior and one to posterior points of scalp as before.

December 5th. Improved feeling to-day, for the first time since about the 20th of November, distinguished day-light.

December 8th. Still further improved in feeling, appetite and strength—quite free from pain. Sleeps well without anodyne; bowels regularly open without medicine, some flatulence yet, but slight; can see persons moving about the room.

Feb'y 20th. He has steadily improved; can see to ride over the country, and to walk about town; as heavy as ever he was, and feels as well as he ever did.

Shelbyville, Ind., July, 1852.

ART. II.—*Two Cases of Fatal Injury of the Head.* By ROBERT DURRETT, M. D., Resident Graduate of the Louisville Marine Hospital, for 1851.

Miller in his Practice of Surgery says, that "between pure examples of concussion and compression of the brain, there is no difficulty in drawing a sufficiently broad distinction in practice as well as in theory. The one, a case of syncope; the other, one of coma. In concussion, the symptoms immediate; insensibility incomplete; the organs of special sense capable of being roused; the muscles contractile, and the limbs under strong stimulus, undergoing movement; the breathing soft and gentle; the pupils not uniformly dilated, though insensible to light; the pulse rapid, small, indistinct, perhaps for a time imperceptible; vomiting; no involuntary evacuations; the skin cold, pale, and shrunk. In compression, the symptoms not necessarily immediate; insensibility complete; the organs of special sense incapable of being roused; the muscles relaxed, paralyzed; the limbs motionless, until cessation of the state of compression, and advance of the inflammatory process; breathing labored, slow, and snoring; the pupils dilated and insensible; the pulse slow, distinct, perhaps full, sometimes intermittent; no (or seldom) vomiting; feces passed involuntarily; retention or dribbling urine; the skin warm, and often bedewed by perspiration."

The history of the two following cases does not accord with the above quotation.

John Convoy, aged thirty, a marine, was admitted into the surgical ward of Louisville Marine Hospital, March 23d, 1852. He gave the following very unsatisfactory account of himself, to wit: That he had been drinking for several days, and either fell and injured himself or received blows from an unknown source. Upon examination, found exquisite tenderness over the spine between the shoulders,

which was relieved by the application of wet cups. The patient complained a little of his head, tongue was good, pulse about 60, natural in volume, pupils susceptible to light. I supposed the patient to be laboring under the effects of a debauch. Ordered a purge.

March 24th. Patient still unable to tell how he was hurt, but thinks that he was struck by some one whilst drunk; seems to answer all questions propounded intelligibly, complains of constant and distressing head ache; no marks of external injury. Ordered patient's hair trimmed, and cold water applied to the head.

March 25th. Patient sleeps almost constantly, sings and talks in sleep; when aroused, which is done with difficulty, answers questions directly; pulse 70, pupils susceptible to light. Having heard to-day through friends of the patient that he had been struck by the mate of the boat several times, supposed the patient laboring under inflammation of the brain, the result of concussion caused by the infliction of the blows. Ordered submur. hydrar., grs. ij, every three hours, the continuation of cold water to the head.

March 26th. No perceptible change, except slight opisthotonos when patient attempts to move.

March 27th. Opisthotonos continues; patient walks across the ward to the privy.

March 28th. Opisthotonos continues to increase in severity, pulse less full, more frequent, skin dry, rather warm. Ordered, a blister to the back of head and neck.

March 29th. Stopped mercury; patient ptyalised; slight delirium, decided opisthotonos, pulse 100, intermitting; bowels moved by medicine.

March 30th. Patient died with the severest convulsion I have ever seen. On a post-mortem examination, found a fracture almost throughout the entire extent of the lambdoidal suture on the right side; between two and three ounces of coagulated blood immediately beneath the frac-

ture, between the dura mater and cranium; over an ounce of serum in the right ventricle; lymph at the base of the brain, and softening of the spinal cord. The patient's intellect remained unimpaired until within a few hours of his death, showing that the cerebellum is capable of sustaining the gravest injury without materially affecting the mind. It is very difficult, and in some cases almost impossible, to make out a correct diagnosis in injuries or diseases of the brain. Many of the most prominent symptoms of compression may be present during life, and yet autopsies reveal none of the pathological changes, which the symptoms are supposed to indicate, as in the following case.

Allen Smith was admitted into the Hospital, March 15, 1852, one hour after the reception of two incised, and one contused wound upon the head, which felled him insensible to the ground. The patient had retention of urine, dilated pupils, warm extremities, partial unconsciousness, involuntary fecal discharges and stertorous respiration, for several hours before death. A careful examination of his head did not enable us to account for his death, which occurred on the 17th of March, thirty-six hours after injury. We found considerable engorgement of the venous system, and some serum in the ventricles; but not more than we usually find in autopsies—not so much as we invariably find in persons who die of *mania a potu.* I give these particulars because Smith was a man of very intemperate habits, and at the time he received the blows was, without doubt, the subject of delirium tremens.

Louisville, 1852.

Art. III.—*A Case of Glanders in the Human Subject.* By Josiah T. Watts, M. D., of Enterprise, Miss.

Axum Permenter, a young man about twenty-seven years of age, stout and healthy, of rather intemperate

habits, had been for some time a teamster on a railroad, sleeping on the ground at night. Had a horse to die of the glanders, and in about ten days afterwards, began to complai i of an unusual pain or fullness in the head, general lassitude and weakness of the whole system. Continued in this condition for three days without applying for medical aid. I saw him on the 23d of June, he complained of pain in his bowels, and had a thin bloody mucous discharge from the bowels. Believing it to be diarrhea, (for a good many of the family were suffering with this complaint at that time,) I gave him calomel and opium, conbined with kino. The diarrhea ceased under this course of treatment, and he began to improve for two days. He then complained of pain just over the right eye, with a headache; pulse full and bounding, tongue coated with a white fur; tip red, great difficulty in projecting it, the whole nervous system in a complete tremor. Bled him copiously from the right arm, applied a blister to the back of the neck, which discharged freely up to the time of his death. Ordered 10 grs. calomel, with 2 of ipecac at bed time; at 1 o'clock at night, gave ¼ gr. sulphate of morphine, under which he rested well.

24th. Next morning bowels opened twice; no fever, slight head ache, right eye swollen, and also the right nostril. Ordered during the day, quinine, nitre, and paregoric combined; skin became moist after the second dose; at night a Dover's powder. Patient rested pretty well.

25th. Pulse soft, tongue unchanged, bowels moved once; the right side of the face very much swollen, so much that he could not see out of the right eye; right nostril closed; a slight bloody yellow water issued from the eye and nose. Applied cloths wrung out of cold water medicated with acetas plumbi; gave morphine and calomel.

26th. Patient rested badly; face continues to swell; the left side swollen as much as the right; cannot see at

all; gets his breath through his mouth alone; a yellow discharge mixed with blood, from both his eyes and nostrils, removes the cuticle wherever it touches; has to be propped up in bed on account of the discharge from his nostrils, which is swallowed when he is in a recumbent posture; the nose dark colored, with small ulcers over it; difficulty of swallowing; in attempting to drink water about half runs back through the nostrils; the discharge is exceedingly offensive, and similar to that of glandered horses. Saw him again in the evening—condition as in the morning, except that his throat is much swollen; he can scarcely utter a single word, owing to the swelling of his lips; the discharge still continues from both his eyes and nostrils as before, but is not now sanguineous. Complains only of a difficulty in breathing; the discharge is extremely offensive; nose almost black; neck swelling very fast.

27th. Died at 8 o'clock, A. M., the discharge continuing from his nose and eyes until he died.

No post-mortem examination was made. Several persons present at the time of his death remarked, that the odor of the discharge was the same as that which they had often perceived about horses with glanders.

June 30, 1852.

ART. IV.—*On Vital Organic Chemistry.* By C. CALDWELL, M. D., Late Professor of the Institutes of Medicine in the University of Louisville.

[This article was intended as a Report to the American Medical Association, but its venerable author was unable to attend the meeting of the Association, and his views are submitted to the profession through the pages of this *Journal.*]—*Editors Western Journal of Medicine & Surgery.*

At a meeting of the American Medical Association, at

Cincinnati, May, 1850, the following resolution was adopted:

"*Resolved*, That Dr. Caldwell be requested to take into consideration the subject of *Vital Organic Chemistry*, and report to the next meeting, whether, in his judgment, it can be justly called a *branch of science*, and, if so, how far a knowledge of it can be rendered available to the welfare of man."

Were it not for one obstacle, the preparation of this Report would be a humble and comparatively an easy task. But that hindrance renders it complex a d difficult.

The obstacle alluded to is the difficulty of ascertaining a correct and satisfactory knowledge of the subject of the report, apprehending and definitely comprehending the precise import of the phrase "*Vital Organic Chemistry*."

This is not less than the twentieth—perhaps than the fiftieth time I have written, lectured, debated, or in some way exercised my mind in the consideration of the same subject. And on each repetition of the exercise, I have endeavored in vain to acquire a knowledge of the interpretation applied by the members of the chemico-physiological sect of the medical profession, to the expression "vital organic chemistry." For it is to that sect alone that the expression belongs. By the vital sect it is disowned as *an error*, or a *nullity*. To explain myself more fully on the subject.

In the year 1844, when engaged in a controversial correspondence on this topic, I proposed to my antagonist for solution, the following plain and pointed interrogatories:

"What is the genuine meaning of the phrases "*Animal Chemistry*," "*Vegetable Chemistry*" or "*Vital Organic Chemistry?*" For I presume the import of the three forms of expression is the same. The attribute of *vitality* must b involved in each of them. For vegetables and animals are equally *vital*.

"Do the advocates of the doctrines of animal, vegetable, and vital organic chemistry contend, that, in the *formation*

of living animal and vegetable tissues and secretions, the forces called *chemical affinities* or *attractions* and *repulsions* take part, as positive and efficient agents, in like manner as they do, in the modification of various sorts of dead matter? In the formation, for example, of a leaf, a blossom, or any sort of fruit; and in the formation of any animal fluid, tissue, or organ, of the blood, the bile, the urine, a muscle, the eye, the kidney, the liver, the brain, or any other kind of organized and living animal or vegetable matter—is it contended, that, in the formation of these products, the chemical forces act as they do in the formation of granite, marble, siliceous crystal, sulphuric acid, sulphate of iron, or any other sort of dead matter belonging exclusively to the mineral kingdom?"

Such are the interrogatories, which, to ascertain, if practicable, the meaning attached to the phrase "vital organic chemistry," by the chemico-physiological sect in medicine, I proposed through the press, eight years ago; and though the production containing them has been read by multitudes, they remain unanswered. Nor do I think it probable that they will ever be reduced to a different condition, by any of the members of the chemico-physiological sect. Nor are those gentlemen without a reason I suspect for remaining silent. An answer, whatever might be its spirit or matter, might be shown to be prejudicial to either their hypothesis, their own judgment or consistency, or to the three combined.

But an explanation, real or reputed, of the phrase in question, I must receive from some source, else it will be impracticable for me to proceed with my report; because I cannot rationally report or comment in any way on that to which I am a stranger. I shall therefore endeavor to derive some portion at least of the intelligence I need from the writings of Professor Liebig, who bears to the chemico-physiological sect the high and authoritative relation of Grand Master. His *dictum* on the subject, I shall on that

account regard as authentic. From his celebrated work entitled "Animal Chemistry," or "Organic Chemistry," I extract the following passages, and could extract dozens more tending to precisely the same purport.

"In the animal body we recognize as the *ultimate cause of all force*, only *one cause*, the CHEMICAL ACTION, which the elements of the food, and the oxygen of the air exercise on each other. The only known ultimate cause of *vital force*, either in animals or in plants, is a CHEMICAL PROCESS. IF THIS BE PREVENTED, THE PHENOMENA OF LIFE DO NOT MANIFEST THEMSLLVES. * * * * All vital activity arises from the mutual action of the oxygen of the atmosphere and the elements of the food."

"Physiology has sufficiently decisive grounds for the opinion, that every motion, every manifestation of force, is the RESULT OF A TRANSFORMATION *of the structure or of the substance of the animal body.*"

[But that motion or transformation, I reply, is effected, not by a chemical force; but by a superior force, which *controls* that of chemistry, and commands, modifies, and adapts to its own nature the transformation and its effects.—*Au.*]

"In the processes of nutrition and reproduction, we perceive the passage of matter from a state of motion to that of rest, (static equilibrium.) Under the influence of the nervous system, this matter enters again into a state of motion. The *ultimate* causes of these different conditions of the *vital force* are *chemical forces.*

"The power to effect transformations does not belong to the *vital principle.* Each transformation is owing to a disturbance in the attraction of the elements of a compound, and is consequently a *purely chemical process.*

"The combinations of the chemist relate to the change of matter, *forward and backward to the conversion of food into the various tissues and secretions, and to the irmetamorphosis in lifeless compounds.*"

[The only common sense construction of this passage is, that the same forces or powers of action, which at one time produce a *living*, at another produce a *dead* form of matter. In other words, that there is no difference between a *living* and a *dead result*, and therefore, that *difference* and *identity*, *opposition* and *concurrence* are the same.—*Au.*]

"How beautifully and admirably simple, with the aid of these" (*chemical*) "discoveries appears the process of *nutrition* in animals, the formation of their organs, &c.

"The self-regulating steam-engines furnish no unapt image of what occurs in the animal body.

"The animal body, in regard to the production of *heat* and *force*, acts like one of these engines.

"The carbon of the blood, which is converted into carbonic acid within the body, must give out exactly as much heat as if it had been burned *in the air*, or in oxygen gas.

"If chemical action be excluded, as a condition of *nervous agency*, it means nothing else than to derive the presence of *motion, the manifestation of force, from nothing.*

"The most decisive experiments of physiologists have shown, that the process of *chymification is independent of the vital force*; that it takes place in virtue of a purely chemical action, exactly similar to those processes of *decomposition and transformation*, which are known as PUTREFACTION, FERMENTATION, or DECAY."

[Had I time to comment on this passage, I would undertake to establish decisively three points, both interesting and important:

First. That chymification is not a *chemical*, but a *vital* process.

Second. That no vital function or form of action bears the slightest resemblance to putrefaction or decay. And

Third. That, instead of benefiting genuine physiology by his experiments on St. Martin, Dr. Beaumont has seriously injured it.—*Au.*]

"The power of elements to unite together, and to form peculiar compounds, which are generated in animals and vegetables, is CHEMICAL AFFINITY.

"We should not permit ourselves to be withheld, by the idea of a VITAL PRINCIPLE, from considering, in a CHEMICAL POINT OF VIEW, the process of TRANSFORMATION of the food, and its ASSIMILATION by the various organs.

"We know that an organized body cannot *generate* substances, but only change the mode of their combinations, and that its SUSTENANCE and REPRODUCTION depend upon the CHEMICAL transformation of the matters which are employed in its nutriment, and which contain its own constituent elements. Whatever we regard as the causes of these transformations, the act of transformation is a PURELY CHEMICAL PROCESS."

Such are the extracts I have derived from Professor Liebig's Animal Chemistry. And, as heretofore mentioned, I could derive from it, I know not how many more of *the same import.* And its author is of course, as already stated, the oracle of his sect, whose responses and dictates few of his followers venture to contest.

Now, although these extracts do not of themselves amount to a real definition or even interpretation of the expression, Vital Organic Chemistry; they include the elements, out of which one may be constructed. They show conclusively, that Professor Liebig, however he may occasionally express himself, on the subject of a vital principle, considers and virtually declares every process, whether general or special, that is performed in the living system of man and other animals, and in vegetables also, to be performed exclusively by *chemical agency.* They make it clearly appear, that he regards both the *composition and decomposition*, the *building up* and *pulling down* of living organic matter, as a *purely chemical process.*

By chemistry alone, according to his teaching, is the food of man and other animals converted into chyme, the chyme into chyle, the chyle into blood, and the blood

into nerve muscle, bone, and every other tissue, as well as into bile, saliva, urine, cutaneous perspiration, and every other secretion and excretion of the system. In truth, from the showing and acknowledgement of the Professor himself, not only, according to his creed of human construction, is man originally formed by chemical agency; he is subsequently, in his whole economy, as exclusively governed and kept in play by it, as is a "*self-regulating steam engine.*"

Nor, under the light of a rigid and correct analysis of the subject, is this representation less applicable to his *mental* than to his *corporeal functions.* Chemistry is virtually made to give birth, modification, and sustenance to the whole of them.

For if there be in existence a more unscrupulous system of materialism than that contained in Liebig's Animal Chemistry, I know not where it is to be found.

While one sacred writer declares man to be, in his compound capacity, "fearfully and wonderfully made;" and another that he is 'made but a little lower than the angels,' the Professor of Giessen, in his account of him, makes him appear but little higher in his nature and being, than a German stove, or a Belgian beer-barrel! Had not experience and observation therefore abundantly taught me, that true christianity and religious partyism of a ferocious cast are necessarily in a state of belligerency toward each other, it would surprise me to find in the train of his followers and defenders scores of sulphur-lipped fanatics and bigots.

Nor is Liebig the only celebrated chemico-physiologist that represents man as a chemical product. Muller, Roget, Prichard, Carpenter and others of the same school, have maintained the same notion.

Muller, in a special manner, asserts that the seminal or generative substances which the male and the female contribute toward the immediate production of the embryo

(the *semen masculinum* to be planted and the *feminine ovariaas the soil* to receive and nourish it) to be "*produced from the proximate compounds of the blood, by a chemical process.*"

On this ground the embryotic ingredients contributed, compounded, and constructed, by the generative process, are also of necessity chemical. And so must be the issue they jointly engender. Nor, in compliance with an irreversible law of nature, can the *chemical embryo*, thus begotten and sustained, fail to grow up into *a chemical man.* So utterly ludicrous is the web of false doctrine, in which every thorough-going chemico-physiologist necessarily entangles himself! And this is as applicable to Roget, Carpenter, and Prichard, as it is to Muller.

I am now prepared to enter on the business of my report; because I now know respecting the subject of it all that I am at present destined to know.

From the contents of the writings of Liebig, Muller, and the other chemico-physiologists referred to, concurring with sentiments I have myself long entertained, and often expressed, I shall define Vital Organic Chemistry to be the groundless notion that—

Genuine chemistry can and does, without the cooperation and aid of any other force or principle of action, generate and sustain living organic matter, in the forms and capacities of vegetables and animals.

True, when the advocates of this notion speak of it in general and theoretic terms, they connect with it a superior controlling and plastic force, which they denominate *vital.* But when they describe specially and practically any function of living organized beings, they refer its production to a force *purely chemical.* And, singular as it may appear, it is notwithstanding true, that several, if not the whole of them, when pressed on the subject, pronounce the superior and plastic additional force to be the immediate influence of the WILL OF THE CREATOR—an act

or rather an artifice amounting in them to an abandonment of reason and science, and the adoption, as a substitute, of an expedient of rank superstition and fanaticism. It is virtually proclaiming every function of animals and vegetables to be something *above the operation of natural causes*, and *therefore miraculous*—all which shall presently more fully appear. Meantime, from the considerations on the subject hitherto adduced, I am inclined to report, that,

In my judgment, Vital organic chemistry cannot be justly called a branch of science. It is a MEANINGLESS NOTION.

And this report I shall endeavor further to substantiate by conclusive arguments. To execute this measure the more effectually, my first act shall be, to attempt the refutation of the most distinguished chemico-physiologists, who, in their writings on the subject under consideration, have offered sentiments contradictory of mine. And the most effectual mode of overthrowing an opponent being to set him irreconcilably at war with himself; my design is to refute my antagonists, by arguments derived from their own productions on the topic I am considering. In other more express and forcible terms, I shall, by making it clearly and conclusively appear, that their own arguments (if such they may be called) are self-destructive, I shall affix on them the felony of *literary suicide*. And, as the most prominent and anthoritative of the corps, I shall begin with Professor Liebig. In his work on Animal Chemistry, I find the following passages, which will be perceived to be directly opposed in sentiment to those which have been just quoted from the same volume.

"A rational physiology cannot be founded on mere reaction; and the living body cannot be viewed as a *chemical laboratory*.

"With all its discoveries, modern chemistry has performed but slendor services to physiology and pathology.

"In the animal ovum, as well as in the seed of a plant, we recognize a certain force, the source of growth, or in-

crease in the mass, and of reproduction or of supply of matter consumed. * * * This force is called the *vital force*, the *vis vitæ*, or *vitality*.

"He is not the true chemist, who endeavors to apply to the animal organism his notions derived from purely chemical *processes*. He has not the remotest intention of undertaking the explanation of any really *vital phenomenon* on *chemical principles*.

"In what form, or in what manner the vital force produces mechanical effects in the animal body, is altogether unknown, and is as little to be ascertained by experiment as the connexion of chemical action with the phenomena of motion.

"So it is with vital force, and with the phenomena exhibited by living bodies. The *cause* of these phenomena is not *chemical force*; it is not electricity or magnetism. It is a *peculiar force*, because it exhibits manifestations which are formed by *no other known force*.

"The vital force appears as a moving force or cause of motion, when it *overcomes* the chemical forces, cohesion, and affinity, which act between the constituents of food * * * * The vital force is manifested as a cause of motion in *overcoming* the chemical attraction of the constituents of food, and is also the cause which *compels them to combine in a new arrangement*."

Though I could adduce, from Animal Chemistry, scores of other passages to the same purport with the foregoing, the labor would be superfluous. Those already quoted more than neutralize—they utterly *demolish* the work, as a chemico-physiological production. To prove that a publication is *self-inconsistent, is to destroy its reputation and influence*—the penalty thus irrevocably inflicted on Professor Liebig's book.

I shall now expose in the same way, to the same extent, and on the same subject, the errors of Dr. Roget and Dr. Carpenter.

"However," says the former of these distinguished

writers, the laws which regulate vital phenomena may appear, on a superficial view, to differ from those by which the physical changes taking place in inorganic matter are governed, still there is really no essential difference between them." * * "It may, in like manner, be contended, that the affinities which hold together the elements of living bodies, and which govern the elaboration of organic products, *are the same* with those which preside over inorganic products."

So much for that gentleman's views, as a chemico-physiologist, in relation to the production and maintenance of living organic matter. Let us now contemplate his views on the same subject, as a vitalist, compare them with those just stated, and judge of the contrast.

"The foundations," he says, "of the edifice" (of the chick in the egg) "are laid in the homogeneous jelly, *by the efforts of the vital powers.*" "At first," he continues, "all the energies of *vitality* are directed to the raising of the fabric, and to the extension of those organs, which are of greatest immediate utility; but still having a prospective view to farther and more important ends," and so on to the close of the process, the whole work of developing, fashioning, and locating the fœtal organs being assigned exclusively, to the "*efforts of the vital powers,*" and to the "*energies of vitality;*" *chemistry having, from beginning to end, no agency in the work.* In another part of the same publication, Dr. Roget correctly asserts, that the products of the *vital powers*, in their *formative processes*, are uniformly *organs, designed and fitted for the performance of given purposes;* while the products of chemical forces are uniformly both *organless* and *designless.*

And, for a further distinction equally essential, between vital and chemical products, he might have correctly added, that the former are uniformly bounded by curve or waving lines; and the latter by straight lines and angles.

Dr. Carpenter, whose ability and celebrity as a writer, neither need commendation from the pen of a friend, nor dread censure from that of a foe; when under the influence of his chemico-physiological mood, writes as follows:

"Reason has been already given for the belief, that the affinities which hold together the elementary particles of organized structures are not different from those concerned in the inorganic world; and it has been shown that the tendency to *decomposition*" *(putrefaction)* "after death, bears a very close relation with the activity of the *changes that take place in the part during life.*"

But, when in the mood of a vitalist, which occasionally visits him, the same gentleman writes to the following purport:

"Organization, and vital properties are simultaneously communicated to the germ by the structures of its parents. Those vital properties confer upon it the means of itself assimilating, and thereby organizing and endowing with vitality, the materials supplied by the inorganic world."

And in the same spirit he further says:

"The agency of vitality does not change the properties of the elements" (of the food and the blood) "but simply combines them *in the modes which we cannot imitate.*"

The late Dr. Prichard, one of the most keen, close, and elaborate thinkers and writers on matters of medicine that England has produced, was several years ago a bold and thorough-going chemico-vitalist. But a further examination of the subject now under our consideration, induced him so far to alter and modify his views, that, in one of his later publications, he thus expressed himself:

"We may, if we choose to do so, term the cause which governs the organization and vital existence a *plastic prin-*

ciple; but it is a principle *endowed with intelligence* and *design.* It is in fact nothing more than the ENERGIES OF THE DEITY. The development of generic, specific, and individual forms, as well in the vegetable, as the animal kingdom, can be accounted for only, by ascribing it to the universal energy and wisdom of the Creator."

Yet was the Doctor, I say, at one time, a firm believer in the doctrine (or rather *notion*) of *chemical vitality.* But having, from his own superior penetration and sagacity, become thoroughly convinced of the fallacy and preposterousness of that hypothesis, and perceiving the absolute necessity that a plastic power and not a mere power of *chemical affinity* must be employed in the construction of living organized matter—under these impressions, actuated by some unaccountable crotchet, which, at times, takes possession of the most distinguished minds, instead of looking for the requisite plasticity in a *secondary* cause; he referred it at once to the GREAT CAUSE of causes; thus representing the development of even a blade of grass or a leaf of mucor as a *miraculous event!*

Having thus shown that the leading chemico-physiological writers referred to have, by self-contradiction, refuted themselves, in relation to the subject under consideration, I shall endeavor to strengthen that refutation, by a few arguments drawn from my own resources. And the matter of those resources will be derived from the conditions and manifestations of the human body in a living state.

But prevented, by a want of time, from including in my discussion of the subject, all the functions of the system of man, and making it appear that a belief in the existence of vital organic chemistry, as a branch of science, is forbidden by the nature and economy of the whole of them; I shall confine my remarks principally to one of the most prominent and important of them, and the attribution of whose *origin* and *continuance* to the influence of chemical

forces involves less absurdity than a similar notion respecting the attributes of any other sort of vital action.

There are especially two of the functions belonging to the system of man and other warm blooded animals, which constitute at present the chief topics of contention between the vital and the chemico-physiological sects of the medical profession. They are *calorification* and *chymification.* And I am in my own unqualified opinion, prepared to show, beyond denial or doubt, that neither of them is explicable on chemical principles. But as I cannot, in my consideration of the question before me, embrace them both, I shall select the former. And Liebig being, as already stated, the leader of the sect opposed to me, I shall confine my remarks principally to his dogmas on the function to be considered. Should I satisfactorily establish the insufficiency of them, my work will be done. He being vanquished, his followers may be expected to surrender at discretion.

The fundamental assumption of the Professor is, that the heat of the human body and of the bodies of other similarly organized animals, like the heat of a German stove, is the result of combustion. And in this he is correct; except that, in the former, carbon and hydrogen are both burned; but in the latter carbon perhaps alone. Another of his dogmas is, that the same amount of carbon, whether burned *in* the human body, or *out* of it, always evolves the same amount of caloric, and produces of course the same degree of heat. But in this opinion I shall endeavor to show hereafter, that he is incorrect. I shall unquestionably show, that, if his opinion be not, in all respects, essentially unfounded, it certainly involves at least one very striking phenomenon utterly inconsistent with the principles he advocates.

In his fundamental propositions and his comments on them toward the establishment of his doctrine of animal heat, he has laid down one in particular, and contended

for its validity, which I must specially notice; because it is, in my judgment, as groundless as it is important—being, in both respects, of a high order.

That their bodies may retain the requisite temperature, it is necessary for the inhabitants of the frigid zones, especially during the winter season, to generate a much greater amount of heat, than it is for those of the temperate and the torrid zones. To enable them to do this, Liebig contends, that in the former region, persons possessing chests and lungs of the same capacity, inhale, in a given time (because the atmosphere around them is colder) a much greater quantity of oxygen, than do those of the latter region, the atmosphere around them being much warmer.

This hypothesis, though plausible, and very extensively, if not universally believed, I pronounce utterly erroneous. And my reasons for thus characterizing it, are so clear and substantial, that I deem it impossible for any one of even common ability of mind to fail, on due consideration of them, to acknowledge their perspicuity as well as their truth. In testimony of this I shall briefly detail them.

In each climate, torrid, temperate, and polar (and this Professor Liebig himself admits) the same quantity of air makes its way into the lungs, in the same space of time. And he farther correctly admits, that by the time the air has reached the interior of the lungs, its temperature is the same (99 or 100° of Fahrenheit) whatever may be the temperature of the surrounding atmosphere from which it is derived. And I myself, (on the ground of experiment) positively add, that however cold may be the atmosphere from which it is extracted, the air inspired has attained the acme of its temperature (99° or 100°) even before it has passed the *rima glottidis.*

No matter then from what atmosphere it is drawn (polar, temperate, or tropical) if the air inspired (being the

same in volume) possesses, when it enters the bronchiæ and their air-cells, the same temperature, it contains also the same amount of oxygen; which again can unite with and burn up only the same quantity of carbon and hydrogen. Nor are these all the identities that characterize it which the occasion urges me to notice.

With entire truth and confidence may I add, that (in direct contradiction and overthrow of the assertion of Liebig) whatever may be the temperature of the incumbent atmosphere, the air drawn from it, by inspiration, possesses when in the lungs the same degree of humidity.

This assertiou I make to encounter and defeat the design and aim of the allegation by Liebig, that, in the warm atmosphere of the tropics, moisture occupies and fills up the place of oxygen in the cold and dry atmosphere of the polar region.

Admit the truth of this position as respects the air *without* and *around* the persons of the inhabitants, it is not true of the air in their lungs. As already stated, the air in the bronchiæ and air-cells is always of the same temperature, whether the weather be warm or cold; and the secretion from the internal surface of the lungs maintains it in the same degree of humidity, whether the weather be moist or dry.

Did the Professor of Giessen moreover understand meteorology as well as he ought to understand it, before undertaking to excogitate a new doctrine in it, he would not need to be told that the driest condition the atmosphere ever attains exists when the harmattan wind blows. And that prevails in its high degree only in the torrid zone, and chiefly, if not solely during the hottest season of the year.

It is not true therefore, that the tropical atmosphere contains less oxygen than the polar, because it superabounds in humidity. It contains less *when* and *because* it is more expanded by its more elevated temperature. For

the amount of oxygen in a cubic foot, yard, or other measure of the atmosphere is always and necessarily (other things being alike) in precise accordance with such expansion. And in the lungs of a living and healthy human being unadulterated atmospherical air is uniformly in the same condition.

The truth of this position, if it be held in doubt, I freely stake on the following experiment.

Let the ablest analytical chemist of the day, (Professor Liebig himself,) during ordinary weather, take from each of two bodies of the atmosphere, one of them at the temperature of zero, and the other at that of 80° of Fahrenheit, a cubit foot of air. Let him heat these two extracted portions to the temperature of 100°. From each of them again let him take an equal quantity by weight or measurement, and accurately analyze it, and he will find in each the same amount of oxygen, and of water in the form of vapor. For, come from what climates they may, equal quantities of atmospheric air, of the same temperature with each other, possess equal quantities of oxygen, as well as of nitrogen and carbonic acid gas, and also of humidity under similar circumstances—of humidity I mean in the air-cells of the lungs.

In asserting therefore that individuals possessed of chests and lungs of the same size, and breathing an equal number of times in a minute, inspire, in a given space more, oxygen in cold climates than in warm—in Archangel than in Calcutta,—Professor Liebig has committed a mistake, which he ought publicly to renounce, and no longer mislead his credulous followers. Yet does the *vital* process of respiration *(because it is vital)* evolve, from an equal quantity of calorific materials, a greater amount of caloric in the winter atmosphere of Labrador, than it does in the winter atmosphere of Panama; or in that of any part of tropical Africa. And this I say it does under the controling influence of a principle or force of a higher

order and character (I might say of a higher order of *being*) which enables it to produce the effect—that effect being indispensable to the economy of living matter. Nor is this the only error, respecting animal heat, of a nature somewhat similar, into which that Professor has fallen.

Because infants possess lungs more voluminous, in proportion to the size of their systems, than adults do, the Professor has asserted that they also evolve by respiration (in the same proportion) more caloric, and therefore possess a higher temperature.

Let the validity of this notion be tested by the following experiment, and I venture to pledge my experience and observation that it will be found wanting.

Expose to a cold and humid atmosphere, under a covering equally light and scanty, an infant and an adult alike healthy and robust for their age, (each remaining equally quiet) and mark the issue. Before the adult will be, in any perceptible degree affected, the temperature and pulse of the infant will begin to decline. On this point unerring instinct correctly informs the inferior animals. Hence the attention and care with which the mother protects her young from the cold. And for want of attention similarly affectionate and wise, thousands of infants suffer and perish. This clearly shows that the quantity of heat evolved is not, in all cases, proportionate to the qnantity of oxygen employed in respiration. And to me the following facts appear to testify to the same purport.

The chest and lungs of some adults are much more capacious, in proportion to the size of their whole systems, than those of others; and they inspire as fully and frequently. They inhale, of course, in a given time, a greater amount of oxygen; and neither their perspiration nor any other discharge by them is more copious. Yet is their temperature no higher.

On the ground of his own experiments, we are assured

by Treviranus, a writer of reputation and credibility, that, in an atmosphere at the temperature of 80½° Fahrenheit, the honey bee exhales three times as much carbonic acid gas, as it does in an atmosphere at the temperature of 50½°. Yet, may we confide in the doctrine of Professor Liebig, and other chemists, the insect inspires, in the latter case, the greatest amount of oxygen; and we do not know, nor have we any reason to believe, that it consumes, in its aliment, less carbon. Nor when the atmosphere is at the temperature of 50½° is the honey bee at all impaired, or in any way wanting in the functions of vitality.

Muller informs us, on the authority of experiments he has performed or witnessed, that some cold-blooded animals inhale *three times* as much oxygen as is requisite to form the amount of carbonic acid gas which they discharge. Will Professor Liebig, or any of his followers inform us, how the superabundant quantity is disposed of by those animals?

Muller again informs us that man exhales a greater amount of carbonic acid gas when the barometer is *low* than he does when it is *high.* Yet does the air contain, in a given volume, a larger portion of oxygen in the latter case, than it does in the *former*, and of each of its other ingredients also a higher ratio.

The human female expires a larger quantity of carbonic acid gas when in a state of pregnancy, than when in an unimpregnated one. Yet does she inhale less oxygen, especially during the latter stages of her pregnancy, on account of the diminished expansion of the lungs, owing to the augmented pressure on them by the enlargement of the uterus.

These facts are derived from the economy of animated nature, in a healthy condition. Let us attend to the phenomena exhibited by it, in the same respect, in a state of disease.

The temperature of a patient, in an advanced stage of

typhus fever, when he is greatly debilitated, is often as high as 105° or 106° of Fahrenheit, and, at times higher. Yet is his respiration exceedingly restricted on account of debility. He inhales, in a given time, less oxygen by a third or fourth part, than he does in health; and from its emaciated condition, his body is equally deprived of carbon.

In cases of asthma and hydrothorax the same is true. Respiration is preternaturally confined, and the temperature at times, though not perhaps always, preternaturally high.

The following passages, on the same subject, are extracts from my "Physiology Vindicated;" a work published in the year 1843; and no refutation or exposure of their errors, possessing the least merit of any description that has ever been attempted.

"But it is more especially by the phenomena of phthisis pulmonalis that the hypothesis of our author" (that now under consideration) "is irretrievably overthrown. When sufferers under that complaint are able to breathe with but a small portion of a *single lung*, when they have not in their entire systems an ounce of fat to furnish carbon, and but comparatively little water to furnish hydrogen; and when the amount of their food and drink (especially the former) is exceedingly small, and none of it oily—under this destitution of all materials essential to combustion—as well of oxygen to consume as of "fuel" to be consumed—they manifest, for weeks and even months, a high degree of febrile temperature, amounting to 105° or 106° of Fahrenheit—while it should, according to the hypothesis we are examining, be far below the human temperature of health. Were there no other argument but this, to urge against our author's notion, that the vital temperature is the result of *common combustion*, it would alone completely demolish it."

Scores of cases of phthisis pulmonalis, of this descrip-

tion, have fallen under my own observation. In corroboration of their truth, the following very extraordinary case of the same complaint (not seen however by myself) is referred to, in a pamphlet, which I published in the year 1844. I here extract a brief account of it, from that production.

The case is recorded on the authority of Graves and Stokes of Dublin, and appears, in the 5th volume of the Dublin Hospital Reports. It is also referred to, in the American Journal of Medicine, vol. 8, year 1831, p. 218, and is as follows:

"Each lobe of the lungs was extensively tuberculated; the pulse beat 126 times in a minute; and the temperature was *highly febrile.* Yet did the patient respire but *fourteen times* in a minute!"

To contend that the temperature of this patient, under this case of disease, was produced by the *common combustion* of carbon and hydrogen, I pronounce preposterous! As well may a disputant exercise his sophistry, in contending for an effect without a cause, or for the existence of two adjacent hills without a hollow!

I well know that, by some of my opponents, it is contended that, in these cases of inordinately *augmented* febrile heat and *diminished* respiration, the excess of temperature is attributable to the *want* of perspiration. But I as well know that the *cause alleged* is far too *limited and feeble* to account for the magnitude of the effect produced. And a fact strongly analogous to this I shall demonstrate by *count and admeasurement*, which, when accurately executed, never deceive.

Nor is the following representation less irreconcilable with the dogma of Professor Liebig. I quote again from a publication of my own, which has also lain uncontradicted, since the year 1844.

"Sir Benjamin Brodie ascertained by experiments, whose accuracy is not doubted, that a rabbit full grown and healthy, expires every half hour, 28 22 cubic inches of carbonic acid gas. And when the same rabbit is destroyed by poison, or by bisecting the medulla oblongata, and artificial respiration is kept up in it, a quantity of the same gas, varying from 20.24 to 25.55 and 28.27 cubic inches continues to be exhaled in the same length of time. Yet does the temperature of the animal in the course of an hour, fall from *six to eight degrees.*

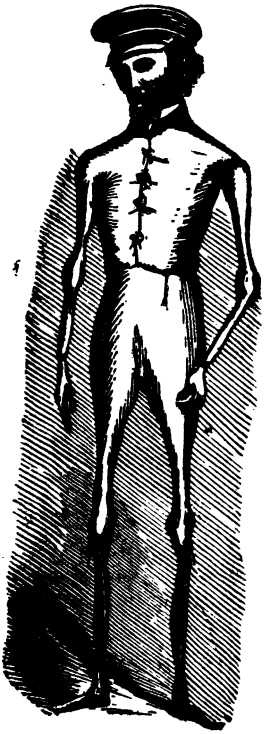
"Some experimenters assert that, under the influence of artificial respiration, the temperature of the dead animal falls more rapidly, than when the process is not employed.

"At any rate, the experiments of Sir Benjamin Brodie, if accurately performed, and correctly reported, satisfactorily show, that when the *vitality* of an animal is extinguished, its temperature falls, notwithstanding the production *in* and the elimination *from* its system, of the usual quantity of carbonic acid, by artificial respiration. And they also show conclusively, that the vital forces, whatever may be their nature or essence, are capable of maintaining animal heat by some agency other than that of *chemical combustion.* And that therefore the economy of a living human body is not identical with that of a German stove, the authority of Liebig to the contrary notwithstanding.

"Facts like the foregoing I could continue to detail almost interminably. And each of them would be alike fatal to the wild, dashing, ill-concerted and worse-supported hypothesis of Liebig and his followers."

But I shall content myself for the present with a statement of the following experiments and the observations accompanying them, which are exclusively my own. As far as my information and belief on the subject extend, the first conception and performance of them were mine. Should their disclosure and record therefore eventuate in usefulness and merit, the issue will pass to my credit. And in case the reverse be their lot, I shall without a murmur submit to the blame.

I here exhibit to the reader the figures of two extraordinary men; Calvin Edson, the "WALKING SKELETON," and J. T. Reed, the "CANADIAN GIANT," whose persons I have seen, and whose characters, habits, and conditions I have personally examined, and carefully studied. Edson's weight was *fifty-seven pounds*, and Reed's but little less than *six hundred;* and the difference between their ages was but inconsiderable in extent—certainly far from being sufficient to mar in any way or degree, the fairness of the experiment and comparison made when I saw them, and now about to be accurately described.



Notwithstanding the prodigious difference in the size of their persons, that in the natural capacity of their chests and lungs was very moderate. In consequence therefore, of the impediment necessarily presented to the play of Reed's lungs by his immense obesity, it can hardly be doubted that his respiration was inferior in compass to the respiration of Edson. Even a presumption that it *might* have been *superior* is *inadmissible.* Let it be conceded however, that the quantities of oxygen they inhaled, in a given time, were equal—as they inspired with about equal frequency.

In neither the kind nor amount of their diet and drink was there any material difference. And of their secretions and excretions the same is true. Singular and perhaps incredible as, to some persons, the fact may appear,

there was no authentic evidence, that Edson discharged from his system, through any of its outlets, a greater

portion of the inherent ingredients of it, than Reed did from the same outlets of his. In a special manner, there was no evidence that he discharged more by *perspiration.*

It may be said therefore, without any material departure from fact, that the two individuals, of whom I am speaking, conveyed into their systems, through the trachea and the œsophagus, the same kind and quantity of matter, solid, fluid, and aeriform. To imitate the language of Liebig—the same amount of *fuel* to be consumed,

and the same amount of oxygen to consume it, for the purpose of heating their "*body stoves*," was regularly appropriated to that end. Nor was there perceptibly a greater escape of their product from one of them than from the other.

But the difference in the amount of matter, of which those stoves consisted was immense—and fatty or oily matter deposited in the apparatus of Reed excepted, the kinds were the same. Nor *did I then, nor do I now*, venture even to conjecture, from what source such a huge mass of oleagenous matter was derived. But *there* it was; and *there* was also the skin and bone fabric of Edson extracted from a similar repository.

It was necessary for Edson to raise his stove, which weighed but little more than fifty pounds to the temperature of 100° Fahrenheit; and he did so; and there the process was checked. And it was equally necessary for Reed to heat his stove, which weighed near six hundred pounds, to the same temperature, else ruin must ensue. And he performed the deed by the same extent of combustion, as his means of action. But, to drop metaphor, and resume directness of speech. With a given amount of oxygen, carbon, and hydrogen Reed raised to 100° of Fahrenheit, the temperature of near *six hundred* pounds of *living human substance;* while, with the same amount, Edson raised to the same height, *but fifty-seven pounds.* Working therefore with the same means, Reed surpassed Edson, in operation and effect, *more than tenfold.*

Such I say was the case, while the bodies of the two individuals were living. But suppose them to be lifeless and cold. In that condition, diffuse through the carcass of Reed the quantity of caloric generated by Edson while living, and through the carcass of Edson the quantity evolved during life, by the system of Reed. What will be the effect? The answer is equally plain and easy. By the operation, the dead body of Reed will not be raised in

temperature *ten degrees;* while the chief portion of that of Edson will be *reduced to cinders*—all in it that is exhalable being turned into gas. Suppose again the two men to be living, and the following experiment to be performed on them.

Let the body of Reed be suddenly deprived of all its caloric, and the caloric of Edson be transferred to it; while to the body of Edson, deprived of *its* caloric, the caloric of Reed is transferred. The result of this exchange is obvious. Edson will be instantly *destroyed and fricasseed by heat*; and Reed *converted by cold into a frozen mass.*

Of these several experiments here submitted, my business is to guaranty the truth; which I deliberately do. Let candid and intelligent men conscientiously apply it, under the guidance and sanction of their knowledge and judgment, to the subject of my report, and I shall cheerfully and confidently await the issue.

The facts I have here clearly, confidently, and veritably stated, make it incontrovertibly appear, that, contrary to the notion commonly entertained respecting it, vital calorification is as little understood, and perhaps more generally and flagrantly misrepresented than any other function of living organic matter.

When therefore a chemico-physiologist presents himself to an audience as a speaker, or to the public as a writer, and gravely assures them, as Liebig, his followers and associates have long done, and as hundreds of teachers are still doing, that vital temperature is the result of *common chemical combustion*—whoever thus acts, becomes by the deed an appropriate object for

"Scorn to point her slow unmoving finger at:"

There are at this time, in Louisville, two females, one of them white and the other colored, weighing each three hundred and fifty pounds. They are also each about fifty-five years old; and their temperature is that of na-

ture and health. I have carefully examined them both, and ascertained from them the following facts:

Until the age of near forty years, neither of them weighed more than one hundred and twenty-five pounds; and their diet and drink then were, in quantity and kind, the same that they are at present. And that their temperature was also the same is not to be doubted; because, as just mentioned, it is now *natural.* And it is presumable that it has been always the same; because they have been always healthy. Nor do they certainly inhale now more oxygen than they did when thirty years younger—probably not so much—their respiration being somewhat impeded and contracted by obesity. Their bulk moreover is nearly three times as large as it was in their youth. Hence, with an equal stock of calorific materials, they raise *now*, at the age of fifty-five, to the same temperature, nearly three times as much animal substance as they did at the age of twenty-five. This neither is, nor can be a deed performed by common combustion, the result of the operation of mere chemical affinity. Nor can I suppress my astonishment, that, at the middle of the nineteenth century, a period so signalized by the knowledge of nature, a contrary belief is seriously entertained by any enlightened and reflecting individual.

My astonishment on this subject would be less than it is, were facts precisely similar to those I have related, less numerous and common than I know them to be. For I doubt whether there can be found, in the United States, a single town or city containing ten or even five thousand inhabitants, where instances of a like description and import do not exist. But to conclude my report.

As a legitimate inference from the facts, illustrations, and arguments contained in this paper, I feel authorized to repeat what I have heretofore said in substance; that "VITAL ORGANIC CHEMISTRY" is a form of expression denoting nothing that actually exists. I am therefore,

compelled to regard it in my best judgment, as mere *verba inana, et preterea nihil*—an empty association of words, which error and a forbidden assumption of knowledge have engendered; and which truth and genuine science disown. All which is respectfully submitted.

P. S.—In my own system a phenomenon has recently occurred, and still exists, which to my mind demonstrates no less conclusively that vital temperature is modified by an influence different from and superior to anything that belongs to the heat of common combustion.

In the great sciatic nerve I suffered recently an uncommonly violent attack of neuralgia, entirely exempt from any augmentation of vascular action, from the effects of which I have not yet recovered. In a special manner the affected limb is not a little weakened in its motive power. And, to say the least, the force and circulation of the blood in it, if not diminished, are certainly not augmented.

Under these circumstances, it was natural to expect, and I did expect that the temperature of the diseased and now debilitated limb would fall below that of the sound and more vigorous one.

But not a little to my surprise, and no less to my gratification and rejoicing, the reverse is now true, and has been so since the commencement of the disease. The sound limb is coolest by at least one degree, if not by one and a half. I say that this phenomenon has occurred *to my gratification and rejoicing.*" The cause of my being thus affected is sufficiently plain. The superior warmth of the diseased limb bespeaks in it a degree of vitality, which promises to minister towards restoring it to health and strength.

Whatever may be the case with regard to others, in contemplating this event, I see or fancy I see in it a striking discrepance between the production of vital temperature, and the temperature of ordinary combustion.

That I may be neither misunderstood nor misrepresented on this subject, I must, if possible be more explicit.

I do not deny that chemical forces constantly and necessarily operate and do much in the living body of man and other animals. On the contrary, I know and acknowledge that they do. But they do not operate *plastically* with respect to *living organized matter.* They neither *organize* such matter, give it figure and organic fitness, nor endow it with life. That work is performed by a higher and controlling power, denominated *vital,* which is emphatically *sui generis.*

Addendum.—The reader must have observed that, in my reply to Professor Liebig, I may have appeared to concur with him in the notion that the blood receives *all* its oxygen immediately from the lungs. But such is not the fact. I do not thus concur. Every article of our diet and drink contains oxygen to a greater or less amount. The blood is therefore supplied with that element through the nutritive no less certainly than through the respiratory function. And animal temperature is developed by vital action in every organ of the body, (the bones themselves not excepted) as indubitably as it is in the lungs.

Hence it was that, in my Inaugural Dissertation, dated in the year 1795 (fifty-seven years ago) I pronounced animal temperature to be "*a secretion*"—or rather a *condition of organic matter, the product of secretory action,* and its abstraction from the system by the application of cold water, cold air, or any other cold substance, to be as *genuine an evacuation* as perspiration, micturition, or the emission of blood by the puncture of a lancet. And antiquated and unfashionable as that doctrine may be now deemed, it was then, for a time, accounted orthodox in Philadelphia; and I still retain it as a sound tenet of my physiological creed. But it was not favored by Dr. Rush, because it clashed with some of his peculiar notions.

I therefore regard animal temperature as the product of a process *essentially and exclusively* VITAL. Nor do I hesitate to pronounce the chemical dogmas of the celebrated Professor of Giessen, when applied to living organic mat-

ter, whether vegetable or animal, as groundless and untenable as the wildest vagaries that ever issued from the tongue or the pen of that prince of boasters, fictionists and notion-mongers, the renowned AUREOLUS PHILLIPPUS THEOPHRASTUS BOMBASTUS DE HOENHEIM PARACELSUS.

I shall only add, that Dr. Woodhouse, a sound and strong minded thinker, and Professor of Chemistry, at the time of my graduation, in the Medical Department of the University of Pennsylvania, and one of the ablest chemists then in the United States, adopted my opinion respecting animal temperature. From the time of the publication therefore, of my Inaugural Dissertation in 1795, to the end of his life, he regularly taught the doctrine that the caloric generative of animal temperature is the product of a *bona fide secretory process*, and its passage out of the body a genuine evacuation.

To conclude, by a summary of certain sentiments I have heretofore expressed. I shall be neither surprised nor disappointed should some of the chemico-physiological sect of medical speculators cavil at and perhaps denounce my remarks on Professor Liebig's work on Animal or Organic Chemistry. I am prepared to find that such fault-finders and disparagers will be inclined to attack and condemn my strictures on that elaborate but (to me) obscure and equivocal production, because its author has not, in any part of it, clearly and fully recognized the existence of a *vital force or principle*, as an *indispensable attribute* of man, and other living organized beings, nor correctly recounted its uses and performances—because in fact he has not described it as the great *plastic power*, which, as the chief executor of the laws of heaven, for that special purpose framed and provided by heaven's supreme LAW GIVER—makes man what he is in structure, and enables him, through his endowment and aptitude, *to do what he does by his efficiency.*

Is any one of the sect referred to prepared to contend that Professor Liebig has sufficiently described and char-

acterized the Vital principle of man and other forms of organized matter, both vegetable and animal, in all the powers and fitnesses it possesses, and in all the achievements it performs in the diversified economy it exhibits in the wide field of organized matter?

I fearlessly reply that he has not. And I with equal fearlessness defy any physiologist of any sect, or all physiologists of all sects, to specify the page or pages of his book or books, in which the Professor has thus acquitted himself—in which he has fairly and fully represented the vital principle, as possessing the numerous high, varied and important attributes it does possess, and as performing the numerous high, diversified and important functions it does perform in the economy of life and organization.

That the Professor has at certain times, and for certain purposes, spoken of the vital principle *in general terms*, with propriety and correctness, is not now, nor ever has been denied by me. But when his remarks involve *particular points and specifications*, his spirit and purport instantly and utterly change; and what he had previously attributed to *Vitality*, he then transfers to *Chemistry*.

In consequence of this versatility of purpose and conduct, his Animal or Organic Chemistry is one of the most flagrantly self-inconsistent, and self-contradictory productions I have ever examined. Hence, in the progressive improvement of the science of anthropology, and other forms of living organized matter, the popularity and influence it once possessed and exercised are as certain to pass from it, as is the life which actuates and has sustained him in a sphere so ample, attractive and commanding, is destined to pass from the mortal fabric of its distinguished author.

The two first pages of my article on "Vital Organic Chemistry" were printed off without my having examined and corrected them. In consequence of this the reader will please to make for himself the following corrections.

In t e third sentence from the top of page 104, for "humble" he will read, *simple*.

And in the fourth sentence for "apprehending and definitely comprehending," he will read,—*of penetrating* and definitely comprehending. C. C.

BIBLIOGRAPHICAL NOTICES.

ART. V.—*The Vital Statistics and Sanitary Condition of Memphis, Tennessee, an Anniversary Address, delivered by appointment, before the Memphis Medical Society, on the 5th of February*, 1852. By GEO. R. GRANT, M. D.

Dr. Grant, when he commenced the investigation, the results of which are embodied in this pamphlet, had the conviction, "that Memphis is one of the healthiest places on the Mississippi river." He believed that a comparison of the sanitary condition of that city with that of "other places in the great valley, and also with other cities in the Union, would conclusively establish the truth of this opinion." He says, he entered with alacrity on the self-imposed task of disproving by figures the charge, that Memphis is a sickly place, but on inquiry he was forced to conclude that, in this instance, common rumor had too much truth in it. We make a few extracts from his interesting address.

The *white* population of Memphis, according to the census of 1850, was 6,369. Of this number, 3,587 were males, and 2,782 were females. There were also, 44 free *colored* males, and 65 free *colored* females. As the deaths among these 109 free persons of color were not kept separate, but included in the returns among the whites as free persons, their numbers must be added to the whites, in order to arrive at the per centage of mortality among the persons, white and colored, composing our white population. Among this white and colored free population, the

returns give 238 deaths; a per centage of 3.67, or 36 in every thousand. From the same authority we learn, that the slave population numbered 2,362; of whom 116 had died; making the per centage of deaths among this class 4.91, or 49 out of every thousand. The average mortality of these two classes together, gives the astonishing result of a fraction over 4 per cent., or 1 death in every 25 living!

The returns of the City and County being kept separate, made it, comparatively, an easy task to examine the *vital statistics* of our immediate neighborhood, and therefrom collect materials wherewith to compare our sanitary condition with that of the people by whom we are surrounded. These statistics show a population in the county, exclusive of the city, of 10,317 free persons, and 11,998 slaves; making a total of 22,315. The deaths among the free were 190, being 1.84 per cent; and among the slaves, 279, giving 2.32 per cent. The average ratio of mortality among our neighbors turn, out to be only 2. 1-10 per cent of the entire population, or one in every 47 living. Here we have proved to us, by unmistakeable data, that whilst one in every 25 had died in town, the people who are separated from us by a mere compass line, have lost but *one*, by death, out of every 47 living. This difference in the mortality of the city over the county, of very nearly *two to one*, is well calculated to excite the surprise of the statist, and the sympathy of the philanthropist; while it offers to the medical philosopher an open and interesting field for etiological investigation.

If the foregoing statements are calculated to astonish us, what will we say when we compare our sanitary condition with that of New Orleans, as shown from the late census returns of that city, the vital statistics of which have been carefully collected by Dr. Barton, and published in the last volume of the "Southern Medical Reports." According to these returns, we find the mortality in New Orleans and Lafayette, *exclusive* of cholera, to be a fraction less than 2 per cent., (1.96) and *including* cholera, it is less than 2½ per cent; being 2.44. This shows a lower rate of mortality, by more than one and a palf per cent. in New Orleans than occurred in Memphis during the same period of time; the facts being collected in a similar manner; ours, by one of our own citizens, who is in every

respect well qualified for the proper performance of the duties the office enjoined.

It would be a fruitless labor to extend our inquiries to other cities of the Union, with the hope of finding in the census returns of 1850, any mortuary statistics at all approximating to the figures exhibited by the recorded deaths among our population, for the period embraced therein. Every comparison of the sort would only be to our disadvantage.

Dr. Grant finds that the mortality in the Memphis Hospital is also greater than in other similar institutions in our country.

The total number of admissions into the Memphis Charity Hospital for the year ending on the 31st of December last, was 474. The books show that 116 of these had died; making a mortality of 24.47 per cent; wanting a fraction only of being one out of every four, or very nearly 24½ out of every hundred! That this is a terrific mortality, will become quite evident by comparing it with the annual mortality witnessed in other Hospitals—some of them in localities confessedly insalubrious.

From estimates made from twelve years' observations by Dr. Playfair, in the Hospitals of Liverpool and Manchester, the mortality was 3.57 per cent., or one in every 28. From a statement published in the January number of the "London Lancet" for the present year, we learn that there had been admitted into all the Hospitals of Paris, during the year 1850, 84,044 patients. Of these, 6,855 had died; making an average mortality of 8.15 per cent., or one in every 12½.

In the Marine Hospital at Louisville, Kentucky, as we are informed by Dr. Rogers, in the December number of the Western Medical and Surgical Journal of last year, the mortality was a small fraction over 7 per cent., or one in every 14.

According to a statement published by Dr. McKelvey, the Surgeon to the United States Marine Hospital at New Orleans, out of 1,116 admissions in the year 1849, there occurred but 64 deaths; making a mortality of only 5.73 per cent., or one in every 17. In the Charity Hospital,—

that immense lazaretto of New Orleans,—the admissions in 1850 numbered 18,676 patients, of whom 1,884 died—being a mortality of 10 per cent., or one in every 10.

From these comparative hospital statistics, it is but too evident that the mortality at our Hospital is more than twice as large as that of the Charity Hospital at New Orleans, which has always been considered, in this quarter, as the great Southern receptacle of the sick and the dying!

As any farther inquiry would serve only to increase the melancholy reflections which naturally intrude, when we compare the picture presented by our Hospital returns, with that of similar institutions in this or other countries, we will leave this branch of our investigation, and proceed to give the returns collected from the records kept by the Secretary of the Board of Health.

These momentous facts established by the researches of Dr. Grant ought to attract the attention of the municipal authorities of Memphis. There can be no doubt that there is a needless mortality in all great cities—a frightful waste of human life which a wise provision might prevent. We gave some details respecting the mortality of Boston in a late number. It appears from a statistical report by one of the municipal officers of New York, recently made public, that the deaths in that city during the year 1851 reached the enormous aggregate of twenty thousand and twenty-four, an increase of five thousand and forty-six, or nearly thirty per cent. over the number in 1850. The increased mortality took place chiefly among children; the increase in adults being 1,264, and in children, 3,782. The number of deaths from consumption during the year was exceedingly large, but the greatest increase was in those classes of disease which are propagated by contagion, and have their causes in the impurity of the atmosphere. The deaths by typhus fever increased from 396 to 977; by scarlet fever from 311 to 627; by dysentery from 792 to 1,113; by diarrhea from 473 to 743; and by convulsions from 1,288 to 1,792.

Compared with London, the mortality in New York is as one in thirty-one to one in forty-eight. A writer in one of the papers of that city, says there is something startling in these facts, and adds:

If there is any truth in official figures, the fact is incontrovertible that, in spite of increased wealth, improved medical skill, and the introduction and almost universal use of that invaluable blessing, the Croton water, the active causes of disease and death are increasing among us; and that the actual duration of life is yearly diminishing.

ART. VI.—*A History of Epidemic Cholera, as it appeared at the Baltimore City and County Alms-House, in the Summer of* 1849, *with some remarks on the Medical Topography and Diseases of this Region.* By TH. H. BUCKLER, Physician to the Baltimore City and County Alms-House. Baltimore: 1851.

This is a very carefully prepared paper, and embodies a large amount of useful information on a most interesting subject. We have only room for a few of the author's conclusions.

Theories Tested.—Before the removal of the nuisances the various cholera theories were tested as far as practicable. Saucers containing solutions of acetate of lead, nitrate of silver and other delicate reagents, were placed on the margin of the pond, and at various other points back of the north wall; and numerous strips of chemically pure paper wet with solutions of these salts were hung out at night over the different pools. Paper prepared with Schonbines' solution of iodide of potassa and starch were also used to test the presence of ozone. Duplicate experiments were instituted in the city at the same time, but without any very satisfactory results in either of the trials, the changes which occurred being nearly alike at the two places.

With the view of testing the cryptogamic and animal-

cular theories, plates of microscopic glass attached to threads by means of sealing wax were dipped in solutions of sugar, starch and gum acacia, and hung back of the north wall and in the cholera hospital. Other plates of glass were covered over with glycerine, remarkable for its property of remaining fluid for a long time when exposed to the air, and these, like the former, were suspended in various places about the establishment. Sugar and starch were selected because of the known tendency of vegetable germs to form on these compounds, and it was supposed if animalcula existed in the air that some of these would of necessity be caught on the moist and tenacious glycerine. These plates of glass, having been thus treated, were carefully examined by Dr. Christopher Johnston, aided by powerful lenses, but he was unable to detect the slightest trace of vegetable germs, animalculæ, or microscopic organisms of any sort.

It has been shown, in the last epidemic, that those who were least exposed to the miasm enjoyed the greatest immunity from cholera; that a partial removal of the fruitful sources of malaria was marked by a great reduction in the number of cases; and that with the entire restoration of the establishment, to a proper sanitary condition, the disease entirely ceased. It is fair then to conclude that for the existence of the local impurities, cholera would never have visited the alms house. But it would seem that the removal of these causes of malaria, not only prevented the further spread of cholera amongst the inmates, but what is more important, that the general health of the establishment has also been greatly improved.

If the history of epidemic cholera, as it first appeared throughout this country in 1832, be compared with the invasion of 1849, it will be remarked, as one of the most striking facts connected with this disease, that it returned to every place which it had visited during the first epidemic, unless during the interval, the locality had undergone some marked change, or entire renovation. And not only so, but the last epidemic, was far more general in its distribution than the first, many places which escaped in 1832, having been visited by the disease in 1849. Moreover, since the last invasion, cholera has shown a much greater disposition to loiter about the malarious regions; this has been the case particularly at many places along

the Ohio and Mississippi rivers. Even within the present month, two years and a half from the commencement of the last epidemic, there has been dreadful mortality from this disease on several plantations in the south-western States.

These remarks are not applicable to Kentucky or Tennessee, nor, we believe, to other western States. Many towns visited by cholera in 1833, have escaped the present visitation, though their condition in all respects save growth continues the same. The epidemic has been less general than it was in its former prevalence. Our observation has been, that the ordinary sources of malaria have hardly influenced it at all.

ART. VII.—*The Annual Address to the Graduating Class in the Medical Institution of Yale College, January* 15, 1852. By ALVAN TALCOTT, M. D., in behalf of the Board of Examiners.

This is an address full of sound and seasonable advice to a class of young gentlemen about to assume the duties of their profession. We do not know where they could go for better counsel than is comprised in the pages of this valedictory.

SELECTIONS

FROM

FOREIGN AND HOME MEDICAL JOURNALS.

Cases Illustrating the Localization of Valvular Lesions of the Heart.. Reported to the Buffalo Medical Association, by AUSTIN FLINT, M. D.—The study of the auscultatory signs pertaining to heart diseases has, within a few past years, been pursued with such success, that physical exploration in this aspect, is now even more demonstrative and simple than in its application to pulmonary diseases. The physical evidences of heart lesions are, in fact, so reliable, and so readily appreciated, that, in almost every instance, the practitioner is enabled to predicate with positiveness, the existence of the latter upon the presence of the former; and, what is of hardly less consequence, he is able to pronounce with equal confidence on the absence of lesions, in cases in which he fails to discover the characteristic signs. If this remark does not apply to all the lesions to which the heart is subject, it is certainly correct with reference to those seated in the orifices, or valves of this organ. *Valvular lesions are uniformly accompanied by some of the modifications of the bellows murmur.* If there are exceptions to this rule, they are so few as scarcely to affect our dependence upon it in practice. And, on the other had. the correlative rule follows, that *if the sounds of the heart are perfectly pure, that*

is to say free from any adventitious sounds, valvular lesions do not exist.

But physical exploration carries us farther even than this. Not only does it furnish precise information respecting the presence or absence of these lesions, but it enables us to designate, in the great majority of cases, the particular orifice, or orifices at which they are situated. This minuteness of diagnosis, which to those who have devoted no attention to the subject seems almost inconceivable, and appears to be a kind of divination, in reality is attained by very simple means, which a few words will suffice to explain.

In the vast majority of cases valvular lesions are situated in the left side of the heart; that is to say, in the mitral, or aortic orifices. Why this is so, we will not stop to inquire. The fact being as it is, it follows that, for ordinary practical purposes, it suffices to determine whether the lesions are in the mitral or the aortic orifice, or in both these situations.

Lesions thus situated are, as has been stated, uniformly accompanied by a bellows sound. Now, the localization of the lesions involves certain points relating to the bellows sound. These points are two in number. The *first* point relates to the part of the chest at which the bellows sound is heard with its maximum of intensity, and the direction, from this part, in which the sound continues to be heard farthest, and most clearly. If a bellows murmur be developed at the aortic orifice, it will be heard with a maximum of intensity at a part of the chest over this orifice. This part is the intercostal space between the second and third ribs nigh to the sternum. The sound will be heard most distinctly and loudest in this spot. It will continue to be heard as the instrument is carried in a direction upward from this part, the sound being transmitted by the walls of the vessels, and the current of blood expelled from the heart. It will be heard farther and louder in this, than in an opposite direction i. e. downward over the body of the organ.

On the other hand, if the bellows murmur be developed at the mitral orifice, the maximum of intensity will be found over the body of the heart, toward the part where the apex strikes against the chest between the fifth and sixth ribs. The sound produced at this orifice is more

readily transmitted to the body of the organ, toward the apex, by means of the tendinous chords and fleshy columns, than in an upward direction over the aorta above the heart; consequently, it will be obscure in the latter situation, and rapidly diminish, or disappear, as the instrument is carried above the heart, while it is distinct and loud over the part of the chest lying directly above the organ, and even below this part.

This is a point of distinction which is attended with no real difficulty in its practical application. The principle involved may be illustrated upon the healthy chest. The first sound of the heart is developed, in part, at least, at the mitral orifice. The aortic is passive in the production of this sound. The second sound is due to the semilunar valves of the aorta, and pulmonary artery. Recollecting these facts, let the stethoscope be placed over the chest between the second and third ribs near the sternum. At this point the second sound of the heart will be heard with greater loudness than over the body of the heart; and the sound will be found to be transmitted more readily above than below this spot. The reason for this is the same as in the case of an adventitious sound developed in the same situation.

But the *first* sound will be heard loudest over the body of the body and toward the apex of the heart, obeying the same law which governs the bellows murmur when produced in the same situation.

This experiment can be readily tried.

The bellows murmur, in much the larger proportion of cases, is associated with the first sound of the heart. Now the first sound accompanies the *systole*, and the second the *diastole* of the organ; hence the first is sometimes called the *systolic*, and the second the *diastolic* sound. Recollecting the relations of these two motive acts to the circulation, it will be perceived that when the bellows murmur proceeds from the aortic orifice, it is occasioned by the passage of the blood in the proper direction of the current through this artery. This is not true, however, of the bellows murmur, accompanying the systole, developed at the mitral orifice. The blood passes through this orifice from the auricle to the ventricle, and this takes place during the diastole. A systolic bellows murmur, therefore, if located at the mitral orifice, necessarily in-

volves the existence of lesions of the valves in this situation rendering the orifice patulous, and permitting regurgitation of blood from the ventricle to the auricle, when the heart contracts.

Whenever a bellows murmur accompanies the second, or diastolic sound, it must proceed either from the current of blood in its normal course from the auricule to the ventricle, or from a regurgitating current from the aorta, owing to insufficiency of the valves at this situation In the latter case, an exception may be furnished to the rule that the sound of a bellows murmur developed at the aortic orifice is more readily transmitted in an upward direction. It is, however, extremely rare that a diastolic bellows murmur is heard; and still rarer, if indeed it be ever true, that a disastolic murmur exists without a systolic. When the former is appreciable, an adventitious sound accompanies both motive acts of the heart, constituting the *see-saw* modification.

The second point involved in the localization of valvular lesions, relates to the *pitch* of the bellows murmur. This point is omitted in some recent works treating of the physical exploration of diseases of the heart. Attention was directed to it especially by Bouillaud, in France, and by Dr. Hope. Why it is excluded from the means of diagnosis by some authors, I am at a loss to understand; for, so far as my opportunities enable me to judge, it is a point of considerable importaace. Dr. Hope doubtless erred in stating that the pitch of sound was affected by distance, but that a distinction in this respect obtains between the sounds produced at the aortic and mitral orifices, is probably true of most cases; and the distinction is easily recognized, especially by one who has what is called a musical ear. The rule as laid down by Hope is, that the mitral bellows murmur has a lower pitch of sound than the aortic. Following the ingenious method suggested by Bouillaud, he represents the diatonic variation in the two sounds by comparing them to a whispered word, on the one hand, and a whispered letter on the other hand. The letter *R.* whispered, will stand as the type of an aortic bellows sound; and the word *who*, of the sound developed at the mitral orifice. On comparing the two sounds produced when this letter and word are whispered, the contrast is sufficiently apparent. These representations

are, of course, only approximations to accurrcy, and are applicable to distinguish aortic and mitral bellows sounds only when they are systolic, i. e. accompanying the first sound of the heart.

The valvular lesions are not always limited either to the aortic or mitral orifice. The valves in both orifices may be diseased in the same case. The application of the same principles, however, will in such instances enable us to determine this fact, by showing that a bellows murmur is developed in the two situations.

After this review of the diagnosis, (which has occupied more space than I had intended,) I will invite the attention of the Association to a brief report of three cases that have fallen under my observation within the last few months, in which I have been able to verify, by autopsical examinations, the correctness of the foregoing principles involved in the localization of valvular lesions. In each of the cases the diagnosis was made and recorded before the death of the patient. I have in my possession the hearts of each patient, which I shall exhibit to the Association in connection with a synopsis of the history and physical signs of the cases respectively.

Case 1.—*Disease of mitral valves, allowing regurgitation; with hypertrophy and dilatation. Autopsical appearances.*

J. D., laborer, aged twenty-one. Came under observation October 17, 1851. The following is condensed from the record made at that time:—Had rheumatism when he was sixteen years of age. Several joints were affected in succession, and he went about on crutches for some time. Recollects having had pain in the chest at that time.

Five months ago first observed that in performing labor his breath was short. He was not able to do full day's work, but continued to labor more or less until the first of the present month.

It does not appear that he has had paroxysms of dyspnœa, except when occasioned by exercise, or acts of coughing.

He has had a short hacking cough most of the summer, with very little expectoration.

Appetite has been poor for the last three months. Feels worse after eating, especially after taking animal food; also after drinking spirits.

His aspect does not denote marked anxiety, or suffering. The prolabia are somewhat livid. The alæ dilate considerably with each act of inspiration. He is not emaciated, and the countenance, irrespective of lividity and dilatation of the alæ nasi, is not morbid.

Pulse is 108. Compressible, somewhat jerking.

Skin warm.

Respirations are twenty-eight, and slightly labored.

No œdema of feet or face, and no evidences of ascites.

Physical Signs.—Dullness of resonance over increased space in precordia. Considerable heaving of chest. Apex of heart strikes between the sixth and seventh ribs, about three and a half inches below the nipple, on a vertical line running through the nipple.

Above, and over most of the præcordial space the sounds of the heart are normal, *except that the first sound is not so much developed as usual. No bellows sound in these situations. At the apex a distant bellows sound is heard accompanying the first or systolic sound. The pitch of the murmur is nearer* WHO *than* R, *but somewhat higher than the former. No bellows murmur appreciable except over apex of heart, and in that situation quite loud.*

In view of the foregoing physical signs, the existence of valvular disease situated in the mitral orifice, permitting regurgitation, was pronounced. The attention of the class then in attendance at the medical college was repeatedly called to the case as illustrating the physical signs denoting localization of lesions in the mitral orifice.

The patient remained under observation until his death, which occurred, January 20, 1852. During the latter part of his life he suffered from dyspnœa, requiring him to remain constantly in a sitting posture. He did not suffer from severe paroxysms of difficult breathing, or orthopnœa. The face presented marked lividity. The pulse was feeble. The impulse in the præcordia became faint and diffused. There was moderate œdema of lower extremities.

Post-mortem Examination.—The pericardial sac contained about six ounces of clear serum, with a few flocculi of fibrin. Pericardium transparent and polished. Heart greatly hypertrophied and dilated. Aortic and pulmonary valves free from disease. A few spots of atheroma in the aorta just above the valves. Tricuspid valves free from

disease. *Mitral valves thickened, presenting calcareous deposit in considerable quantity, the tendinous cords contracted, occasionally marked insufficiency.* Left auricle greatly dialted, and filled with blood partially coagulated.

Case 2.—*Disease of aortic valves. Dilatation. Autopsical appearances.*

J. *Z.*, German, laborer, aged twenty-eight. Came under observation Oct. 28, 1851. The following is condensed from the record made Oct. 29th:—The patient states that he had good health up to two and a half years ago. He was then seized, suddenly, with severe pain in both hip joints, which continued more or less until last Christmas. About Christmas an abscess discharged on the inner part of thigh, eight or ten inches below Poupart's ligament. The discharge has continued to the present time, and still continues. He is able to use both hip joints freely.

In May last he first noticed that he was short winded. Previous to that time he was able to labor several days at a time. Shortly afterward he was obliged to give up work. He experienced at this time, occasionally, pain in the precordial region, but has had none within the last month.

He is too feeble to give a very full previous history.

He lies somnolent, in a semi erect posture, but is easily roused.

Respirations forty-eight, and labored.

Aspect notably pallid, with slight œdema of face. Feet and legs considerably œdematous.

Abdomen meteorized, without the physical evidences of ascites.

Skin cold.

Pulse 108, very small and feeble.

Urine deposits considerable albumen on testing with heat and nitric acid.

Physical Signs.—Flatness on percussion over a space in precordia five or six inches transversely across the nipple. Point of impulse of heart felt about an inch below the nipple, and to the left of a vertical line passing through the nipple. No heaving of the chest. Sounds of heart distant and confused. *A bellows murmur, high in pitch, (as high as* R,) *accompanies the first sound, heard between*

second and third ribs nigh to sternum, and not over the body or at apex of heart.

The diagnosis noted in connection with the record from which the foregoing is taken was, *lesion of aortic valves; increased effusion within pericordial sac, with dilatation.*

The attention of the medical class was called to this case as illustrating the localization of valvular lesions, by physical signs, in the aortic orifice.

Death occurred, in this case, on the 29th October.

Post-mortem Appearances.—Left ventricle and auricle filled with grumous blood, and a few white coagula. Left ventricle dilated to about twice its normal capacity. Thickness of walls but little increased. *All the valves free from disease except those at the aortic orifice, which were studded with vegetations and calcareous deposit.* Considerable atheromatous deposit in aorta above the valves. Effusion within pericardial sac estimated to be between four or five ounces.

Lungs healthy, and not greatly congested.

Liver not diminished in volume. The interlobular veins deeply congested, occasioning a granular appearance of the lobules.

Kidneys congested.

Other organs not examined.

Case 3.—*Disease of aortic valves. Great hypertrophy. Angina pectoris. Autopsical appearances.*

Mr. R. applied to me July, 1851, aged about forty. The following is condensed from the record made July 17th :—Patient was attacked three years ago with severe pain in præcordial region. He was riding at the time, and the pain obliged him to lie down at once. Had always previous enjoyed excellent health. Had never had rheumatism. Since that time he has been subject to repetitions of similar attacks of intense pain in præcordial and sternal regions, shooting down into both upper extremities. The attacks are manifestly those of angina. They occur, more or less, every week, and, of late, once, and sometimes twice daily. The pain is excruciating. It is accompanied and followed by palpitations, and by great dyspnœa. Attacks are brought on by exercise, but they frequently occur without any exciting cause. He has been unable to labor for the past three years. He is thin, but aspect not morbid. Appetite good, bowels regular.

There is no œdema. He is not habitually short-winded. His sources of complaint are the paroxysms described. If these were relieved he thinks he should be well. He has consulted several physicians of the city, and submitted to repeated bleedings with no good effect. He has also had the advice of various empirics.

A loud bellows murmur is developed in the aortic orifice, the pitch as high as R, *the maximum above the heart, accompanying the first sound.*

Pulse well developed, not irregular. He states that the pulse intermits during the paroxysms sometimes, and sometimes not.

The dorsal spine is tender on pressure.

Diagnosis. Valvular disease of aortic orifice.

The following prescription was made at this time:

℞. Ext. Hyosciam, ʒi.
Tinct. Digitalis, f ℥i.
Aquæ, ℥ii.

Teaspoonful three times daily.

Croton oil to be applied to spine.

August 11th. The following is noted:—This patient called and states that he has had no paroxysms of pain since taking the above prescription until last evening, and the suffering then was not so severe as usual. He has taken the medicine until four or five days since. It was then omitted because his supply was exhausted. Two or three days after commencing its use he called to complain of not being able to see minute objects. He was unable, for example, to see letters distinctly enough to read. The pupils were dilated. He diminished the dose of the medicine, and the affection of vision passed off in a few days. The medicine evidently produces slowness and intermittency of pulse. He has made the back sore with the croton oil.

November 10th. This patient called to see me about two weeks since, and stated that he suffered nightly from paroxysms of intolerable pain, with dyspnœa. He was sure that he could not live long, and desired that his body should be examined after death He died on the morning of this date, and the chest was examined late in the forenoon of the same day.

The heart was greatly hypertrophied, being more than double the normal size. The enlargement was most

marked in the left ventricle. The ventricular walls were but slightly increased in thickness. Aorta enlarged for several inches above the heart, and the lining membrane opaque, and roughened by atheromatous deposit, which in several points presented calcareous degeneration. *Aortic valves thickened and much contracted.* Pulmonary valves healthy. Mitral valves slightly thickened, but not diseased sufficiently to compromise their functions.

Lungs healthy.

Other organs not examined.—*Buffalo Medical Journal.*

On Blistering in Gleet. By John L. Milton, Esq.—Not merely will a single blister frequently cure the most prolonged gleet—not merely will it rapidly sweep away all dregs of the disease in its ordinary course, but it will often cure those runnings, which have resisted all known and used methods. I have seen two blisters, with a mild injection or two, at once cure a clap which had defied the most energetic treatment; and *as I never found a case which resisted blistering and injections together, that was not complicated with strictvre, or affection of testicle,* I am slowly arriving at the conviction, *that every case of clap or gleet, however obstinate, may, if uncomplicated, be cured by blistering, singly or combined.*

To illustrate and urge forward this operation by every means in my power—to invite attention to it, that it may be put to the severe test of practice, to attest it by cases which I have collected and watched—to point out the necessity of quitting the beaten paths of treatment, and try a new remedy, more powerful than those in use, is the object of this paper. But as so many remedies have fallen into disuse from the indiscriminate use or misapplication of them, and as so many which are in favor have been arrested on their path by obstacles issuing from the same sources, I must here enter my protest against being supposed to recommend blistering in gleet, unless properly applied, and in the cases I have referred to.

In order that a blister be properly applied, there are some points, which, however trifling they may seem, require as much attention as the leading features of the case.

Where these are neglected, blistering is apt to produce such a filthy excoriated mass, that the patient will not submit to it a second time; whereas, if carefully laid on and dressed, it is, from its being out of the reach of friction, in the ordinary movements of the body, even less troublesome than if on a limb, or the trunk. Before putting it on, the hair at the root of the penis is cut off, and, if the foreskin be naturally retracted, it must be drawn a little forward over the glans. A piece of paper is then to be fitted on the penis, and cut till it exactly covers it, from the root to within half an inch of the mouth of the urethra. This is then laid down on the blister, which is cut out by it, wrapped round the penis, and fastened with threads behind the glans and near the root. The patient should remain perfectly quiet during the time it is on, lest any motion should bring the blister against the scrotum, and vesicate it; but he must not apply it on going to bed, or he will most likely fall asleep, and not awake till the penis is one mass of vesications—a state productive of an unnecessary amount of suffering.

In the milder cases, or where the skin is tender, an hour, or an hour and a half, will be sufficient. The blister is then removed; if there are any vesicated spots, they are covered with pieces of linen spread with zinc ointment, and then a layer of cotton is bound over these, and covered with a piece of linen, kept on by a thread, or, what is better, two very thin rings of vulcanized India rubber.

Where a severer case renders a more energetic employment of the remedy necessary, it must be kept on two or four hours, until free vesication is produced; zinc ointment is then applied.

To protect the penis from friction, a T bandage, with a linen bag sown into the part which receives the penis, or a handkerchief carried round the waist, and dipping in front, so as to receive the penis, and keep it up against the abdomen, is necessary.

The first effect of this application is, to increase the discharge considerably, which then terminates, either by altering its character, becoming ropy and mucous, and finally disappearing in a few days, or by remaining somewhat more persistent, and requiring a few injections, when the penis is so far advanced towards healing, that it can be handled without pain, or demanding even a second

blister. One of the most cleanly and convenient, and least painful forms of blister, is Brown's blistering tissue; it causes much less irritation, and heals much more quickly than the ordinary blister. The blistering fluids, if strong enough to vesicate, caused such pain, that I soon renounced the use of them.

How does this remedy act. By counter-irritation, will, perhaps, be the answer. But if this were the case, why should there be increased action in the urethra for a few days and why should the discharge from the urethra begin to disappear, when the counter irritant-surface is healing up? It would seem as if the organized constituents of the urethra are capable of keeping up a certain amount of over-action for an indefinite time; but that when hurried beyond this by a healthy stimulant, *a rebound* takes place, which leaves them less capable than before of furnishing a secretion, morbid in amount or in quality, or in both. We see something similar in prurigo pubis, where a blister causes an exacerbation of the symptoms, succeeded, however, in some cases, by a healthier state of the skin; in bubo, treated by blister, etc.—*Braithwaite's Retrospect.*

On the Hydrated Peroxide of Iron and Magnesia as Antidotes in Poisoning with Arsenic. By J. Haidlen.—The author has satisfied himself by experiments, that to completely precipitate 1 part of arsenious acid within five minutes by hydrated oxide of iron, 22 parts of the latter are required. Some of which had been kept for a year, had lost more than two-thirds of its power; in another case it had lost one-third of it. As is well known, on account of this property of the hydrated oxide of iron, Fuchs has recommended the mixture of hydrated peroxide of iron and magnesia, *i. e.* the mixture of a solution of persulphate of iron, which yields from 17 to 18 per cent. of the hydrated peroxide dried at 212° F., with excess of caustic magnesia. In the Wurtemberg Pharmacopœia the mixture of perchloride of iron with excess of of magnesia is prescribed. The author finds that both mixtures precipitate the arsenious acid equally as well as the pure hydrated peroxide of iron, so that there is no chemical objection to their use.—*Jahrb. für Prakt. Pharm.*, vol. xxiii. pp. 196–202.

On the Treatment of Cancer by the Lactate of Iron taken by the Mouth and injected into the Veins. By Dan'l Brainard, M D., Professor of Surgery in Rush Medical College, &c. —About two years since I communicated to Prof. Mussey, Chairman of the Committee on Surgery of the American Medical Association, some reasons which I had for supposing that the lactate of iron was possessed of more influence over cancer than any medicine yet known.

I have, since that time, had occasion to prescribe it often with results which, while they confirm the views expressed in regard to its efficiency in checking it, have not shown that it was capable of entirely curing it. This result was to me neither surprising nor discouraging, as I have already formed and expressed the opinion that to effect a cure "the whole of the solids and fluids of the body must be brought under its influence." That this is not effected by the simple introduction of medicines into the stomach is sufficiently obvious, and indeed to be expected, since the medicine, used in that way, is subjected to the action of the same nutrition and absorption under the influence of which the disease has originated. It is necessary to go behind this; and one of the means of doing so is by injecting it into the veins. It is only recently that I have had an opportunity of putting this method to the test of practice.

Case. December 13th, 1851. Wm. H. Plumb, æt. 56 years, Englishman, applied to me on account of a tumour of the left orbit.

He gave the following history of his disease: About 25 years ago he had a disease of that eye, called by his physician cataract, which entirely destroyed the vision, but for which no operation was performed. About five years ago he received an injury of that eye from a stick striking against it, which was slight and gave but little pain. About seven months after this blow, he noticed a tumor, no larger than a pea, at the inner canthus "sending off roots into the eyeball." At this time, the tumor and eyeball were removed together by Prof. Smith, of Baltimore. The wound cicatrized well.

He remained in pretty good health about four years, when a tumor made its appearance at the lower and inner part of the orbit; which, in eight months, attained the size of a large hickory nut. It was then operated upon

again, but at the end of six weeks recommenced to grow, and, at the time of this examination, was of the size of an orange, filling up the whole of the orbit and projecting in front of it. Its surface was nodulated, elastic, pulsating, ulcerated to a great extent, and from this point there oozed a bloody serous fluid. He was thin but not sallow, and his health was not very much impaired. He complained, however, of acute lancinating pains through the orbit and head.

16th. Extirpation was performed in presence of the hospital class. It was found so firmly attached to the lower part of the orbit that it was necessary to remove the periosteum with it, and at the back part it could not all be removed. There remained a muscular mass which bled profusely, and which was so soft as to break under the forceps or tenaculum. After several ineffectual attempts to apply a ligature the actual cautery was resorted to and succeeded. The wound was dressed with lint. No inflammation followed. There was a copious discharge of red serum for a day or two, which gradually became yellow, and afterwards changed to pus. He was put, from the day of the operation, on the use of lactate of iron, gr. v, three times a day in solution.

31st. Injected into his veins f℥j of the following solution: Ferri lactis gr. viij; Aq. dest. ℥j. Carefully filter through paper.

Jan. 3, 1852. Injected ʒij of same solution; 6th, f℥iij thrown in; 14th, ʒijss injected; 22d, ʒij; 26th, ʒij; 28th, ʒij.

Feb. 3d, ʒij injected; 9th, ʒijss.

During the whole of this time the wound cicatrized rapidly. At first luxuriant granulations sprang from the surface, which were repressed by the application of nit. silver. Lancinating pains continued for some time, but gradually diminished and at length subsided.

In six weeks from the operation the cicatrization was nearly complete. In eight weeks he returned home perfectly well.

The question whether the diseased mass was cancer I do not hesitate to decide in the affirmative. Its history and appearance sufficiently indicate this; its interior perfectly resembled the brain of an infant in a vascular state, and under the microscope it exhibited the most perfectly

formed cancer cells. Dr. Johnson, resident physician, fully coincided in this point.

Whether it would have cicatrized without the use of the lactate of iron cannot be determined with the same degree of certainty. Taking into consideration the return, when last extirpated, with the fact that it was afterwards impossible to remove the whole of it, I think the probability of obtaining cicatrization by ordinary means was slight. I should not, however, have thought of performing, or attempting extirpation, but that the patient (who is intelligent and trusting) expressed his desire to be submitted to the treatment when it was explained to him.

I am aware that many surgeons, under the influence of preconceived opinions, may regard such treatment as hazardous. I had fully convinced myself that such was not the case. I have repeatedly thrown gr. x lactate of iron, imperfectly dissolved in an ounce of water, into the veins of a small dog without, in any case, peculiarly bad results.

It will be seen that gr. iij was the largest quantity thrown in at a single time. It was passed in gradually and cold, and, as soon as sensible effects were produced, it was stopped. The effect noticed was a flush of the face, a fullness of the veins of the head, and a tendency to sneeze, which all passed over in a few seconds. The circulation otherwise was unaffected. If the case had not progressed favorably, and it had seemed advisable to change the nutrition more profoundly, I would have had the solution warmed and put it in slowly until its effects were perceptible. then, allowing it to pass over, have repeated it as far as appeared safe.

Up to the time of his departure the injection had been performed nine times, and grs. xix in the aggregate injected. When the activity of the salt is considered, it will be conceded that such a quantity is capable of having an effect on the system b. being thrown into the blood.

In addition to that he has during this period, eight weeks, taken ʒxix of the lactate by the mouth, to what extent this may have been absorbed and carried into the circulation, or what changes it may have undergone, it is impossible to determine.

In case of a cancerous disease seated upon an extremity I should in addition to the two methods of administration resorted to in this instance, infiltrate the whole of the

diseased and the healthy tissue about it with a weak solution of the medicine. This can readily be effected by putting a ligature moderately tight about the member until it becomes œdematous, when by the aid of friction the infiltration and maceration may be effected. I had omitted to mention that all the injections were made into the veins at the elbow.

In submitting this case to the profession. I am far from claiming for it any merit which it does not possess, or drawing inferences which no single case could warrant. It is offered as an evidence of the practicability and safety of maceration through the medium of the blood systematically pursued with active substances, and to invite attention to other means of treating this inveterate disease than those which hitherto have been admitted by consent to be unsuccessful.—*Am. Jour. of Med. Science.*

Morbid Habit of Swallowing Hair; Prolonged sojourn of the foreign bodies in the gastro-intestinal canal. Evacuation of Packets of Hair by vomiting and alvine dejections. (Under the care of Dr. Crawford, Middlesex Hospital.)—Dr. Thompson has alluded, in this journal, to the case of a girl who used to swallow her hair, and had lately vomited packets of it. The patient has since then, passed per anum, a large mass of the same organic product; this circumstance induced us to inquire more minutely into the case, and we learned from the girl the following facts.

She is a servant, twenty-three years of age, now pale and thin, but formerly ruddy and stout, and was admitted Nov. 16, 1851, under the care of Dr. Crawford, with very obstinate constipation. The patient began to menstruate at the age of twelve years, and at thirteen, while in a comfortable situation, contracted the habit of picking off her hair, biting, chewing, and at last swallowing it. She went on satisfying this depraved taste for four or five months, when, being reprimanded, she gave it up and has never resumed the custom since.

Soon after this, the patient began to feel a pain under the false ribs, on the left side, just over the spleen and the large extremity of the stomach. She was treated in various ways, and at different hospitals and dispensaries,

during several years, for this pain, no one, nor herself, suspecting that the above mentioned habit was the source of her malady. The general belief was, that she suffered from a tumour in the vicinity of the spleen; pain in that region, constipation of bowels, and wasting, being the principal symptoms.

At last, about a fortnight before admission, she was seized with fits of vomiting, and, among the rejected matters, a solid concretion, about the size of a walnut, was noticed; but this attracted no attention, until a second and much larger one was likewise brought up in the hospital. The nature of the affection became now apparent, but the constipation was very obstinate, and went so far as to produce stercoraceous vomiting. No more hair was noticed after these symptoms abated, until Jan. 26, about nine weeks after admission, when a very large hairy concretion was discovered in the fæces. It was of the size of the dilated rectum, measured five inches in length, and was of a deep black color. (The girl's hair is of a light tint.) The patient states that she felt this in the right iliac fossa, and she is now under the impression that more hair will be evacuated. The health has of late been rather weak, but the appetite is pretty good, and the intellect clear; but the patient complains of flatus, and of the bowels rolling in knots. This is another and very striking example of the difficulty of treating disease, when we do not know *every particular* of the history.—*London Lancet.*

Deaths from Chloroform.—In the *Boston Medical and Surgical Journal*, May 19th, Dr. A. E. Noble reports the following case:

In this instance he [the operator] gave a drachm on a handkerchief, which the patient inhaled without any apparent effect, and he then added about half a drachm, I watching the effect closely. After inhaling again a few times, she said she felt the effect a *little*, but the operator and myself could not perceive any influence. Yet willing she should think that she would not experience any pain, the instrument was applied, and as the tooth was extracted she raised her hands to her face and exclaimed "Oh!"

then letting her hands drop, she sat erect about six seconds, when her head dropped upon her breast. I felt for her pulse, but there was none. She respired not more than three or four times, and was dead! It did not exceed three minutes from the first inhalation until life was extinct. That the effect of chloroform was progressive in this case, is certain. I could detail many circumstances relating to the case, such as age, complexion, apoplectic predisposition, and other physical matters, but do not conceive it to be of interest in these days of chloroform. I will add that she was a resident of Detroit, but was in this place on a visit.

A fatal case in obstetrical practice is reported to have occurred recently in Georgia, from the same cause. And at Stamford, Conn., Mrs. Wood, wishing to have a tooth extracted, requested the dentist to administer chloroform. He complied with her wishes; after inhaling it a few times she sunk back upon the sofa and expired.

On Chloroform as an Emmenagogue. By David H. Gibson, M. D., Fort Towson, Choctaw Nation.—Having nowhere seen, in the course of my professional reading, any allusions made to the use of chloroform, as an emmenagogue, I am induced to submit the following facts for publication, partly from a desire that relief may be afforded to the suffering, and partly from a sense of professional duty.

Cases 1, 2. Occurring in the same person. In October last, Mrs. W——, having a violent headache, to obtain relief resorted to the inhalation of chloroform. Within an hour after the inhalation (which was but for a few seconds) she was flowing freely, and continued thus for *four days.* There was no irregularity of the function of menstruation in the succeeding month (Nov.), but another attack of headache supervening, she again had recourse to the chloroform, and in a half hour the menstrual secretion made its appearance, the discharge continuing for *five* days. In both instances, the chloroform was inhaled about ten days after the subsidence of her regular periods. Since the last inhalation, she has menstruated at her usual period.

Mrs. W—— is slightly inclined to plethora, general health usually good, aged thirty-five years.

Case 3. In the absence of Mrs. W——, from home, her servant girl, having gotten hold of the chloroform, imitated Mrs. W.'s example. A like result was produced upon the girl, who menstruated for four days. The girl is very healthy and about thirty years of age. The inhalation was never renewed by her. In this case, the chloroform was inhaled two weeks prior to her usual period, at which time she again menstruated. Since then she has menstruated regularly.

Case 4. Miss ——, aged 19—general health excellent —no deviation having ever taken place since her first menstrual period, was, during a visit to Mrs. ——, induced to inhale chloroform, through curiosity to experience the sensations produced by it. In a half hour the menstrual fluid made its appearance and the flow continued for four days. The inhalation in this instance was ten days antecedent to the regular period, with which it did not interfere. Mrs. W——, my informant in regard to the foregoing cases, is an intelligent and reliable lady.

Case 5. Came under my immediate observation. Was called to see Mrs. H——, found her suffering much from suppressed menstruation. To relieve urgent pain, ordered hot hip-bath, from which the patient experienced much relief. Waited three hours after the use of the bath, without recourse to any other means, having decided, as this was an opportune case, to exhibit the chloroform, which was done for *thirty* seconds. In *twenty* minutes after its administration, the patient was flowing freely and continued to do so for three days. Patient is of a weakly constitution, the result of much hardship. Age of the patient, about forty years. This case is the more remarkable from the fact that the patient has not menstruated for more than *eight* months.

The suppression was induced by causes not deemed necessary to relate at present. Prior to the suppression, she had been very regular for many years. Pregnancy has nothing to do with the case, as the patient is not at this time, nor for many years past has she been in that condition.

I regret that I have not a greater number of cases to submit for the consideration of the profession. Being but

a young practitioner, I am desirous that more experienced physicians should give the chloroform a trial, in order more fully, than my position will allow, to test its value as an emmenagogue; and diffident of my ability to account *correctly* for the "modus operandi" of the chloroform in the above cases, I shall without comment submit them to those who have better opportunities for investigation.

Before closing, however, I will present the following case. Miss ——, aged 18 years, had an acute suppression of the menses, upon the first recurrence of the monthly period, subsequent to the age of puberty. Epileptic spasms quickly succeeded the suppression. Three years have elapsed since, and she is yet subject to these spasms, at longer or shorter intervals. Within the last eight months, she was placed under my care At one time, she will menstruate healthily—at another, there will not be the slightest appearance of menstruation—again, a leucorrhœa will take the place of the proper menstrual fluid. Sometimes, the leucorrhœal, as also the menstrual discharge, whichever it may be, will appear a week, sometimes two weeks antecedent to the regular period. Without entering into detail, as regards the treatment adopted in this case, I will submit the subjoined query. Upon the hypothesis, that the chloroform did act as an emmenagogue in the cases already related, how far would it comport with the safety of this epileptic patient to administer the chloroform? This question is predicated upon the belief, that, if the menstrual function were regularly performed, recovery from the epilepsy *might* take place. It is intended that the question shall refer more particularly to the epileptic condition, than to the irregularity of the menstrual function. I have no reason to suppose the existence of organic disease, either of the brain or uterus, in this patient.—*Philadelphia Medical Examiner.*

On the Nutrition of Muscles during their Contraction. By E. Brown-Sequard, M. D. of Paris.—It is generally admitted that when a muscle is in a state of powerful contraction, circulation, and consequently nutrition, are nearly arrested in it.

The well-known fact, that we are only able to maintain

a permanent contraction in any of our muscles for a short time only, has been explained by a loss of strength occurring, from the supposed insufficiency of their nutrition. I have frequently performed a very simple experiment which proves that the cause of the rapid diminution of the power of our will, in that case, does not exist in the muscles themselves.

The experiment referred to has sometimes been made on my legs, sometimes on my arms; and it was conducted as follows: I took a weight in one of my hands, and kept my forearm in a state of flexion, so as to form with my arm an angle of only 25 or 30 degrees. In that condition some muscles, and more particularly the biceps, were in a state of permanent contraction. My ability to maintain my fore-arm in such a position, lasted between eight and twelve minutes. When I found it was completely impossible for me to keep my fore-arm in that position, an assistant applied the wires of an electro magnetic machine to my shoulder and my fore-arm, so as to excite the biceps, and some other muscles. When, without any effort of my will, my fore-arm was maintained, nearly in the same position, during more than ten or even fifteen minutes.

After one or two minutes of galvanization, I occasionally tried again the action of the will, and I found that it was able to act anew.

If the explanation be true, that the muscle is not sufficiently nourished, and loses, in a great measure, its irritability during its contraction, and that for this reason the will becomes unable to maintain the contraction longer than a certain time, then the action of galvanism ought to be incapable of producing the contraction. If galvanism is able to act as it does, it is because the circulation of blood, the nutrition, and consequently the muscular irritability, have been very little diminished in the contracted muscles.

I have made another experiment, proving, also, that nutrition continues to take place in muscles during their contraction. If the communication established by the nerves, between the muscles of a mammal and its spinal marrow, is left entire; and if the circulation is completely stopped in that limb, by an amputation of the leg at the hip-joint, then it is found that the muscles of that leg, under the excitation of a powerful galvanic apparatus, lose their irri-

tability after ten or fifteen minutes. On the contrary, if the same galvanic excitation is applied to the muscles of the other leg of the same animal, it is found that the irritability remains a long time without a marked diminution, and that it cannot be completely exhausted. It diminishes little by little, but never disappears entirely. Therefore, it is evident that nutrition may take place in muscles during powerful contraction.

Four distinct organs are active in the case of a voluntary muscular contraction. 1st. The brain, i. e. the organ of the will; 2dly, the spinal marrow; 3dly, certain nerves; 4thly, certain muscles. Which of these is the one which is deficient in the case of my first experiment? It is generally admitted that it is the muscular irritability. My experiments prove that it is not so. Consequently, it remains to know in which of the three other organs exists the deficiency. It appears to me that it is in the brain, because a great many experiments have demonstrated that the nerves and the spinal marrow, when put strongly in action, remain very active during a long time, provided that the circulation of blood continues in them.

In saying that the action of the brain is deficient, I do not mean the action of the whole brain, but that of the part of that organ which is used in producing the contraction of these muscles, which are put in action.

From the preceding facts and reasonings I think I am justified in concluding:

1st. That it is the action of a part of the brain, and not the muscles, which is deficient, when our will is unable to maintain a permanent contraction in them.

2d. That circulation and nutrition are but little diminished in muscles strongly contracted.—*Ibid.*

A Case of Hydrophobia in the South of France.—M. Guyon, of Valence, in the South of France, has communicated to the Société d'Emulation of Paris, the report of a case of hydrophobia, drawn up in a very graphic manner, and presenting some points of interest. The patient was a nailer, about thirty years of age, who was bitten by a little dog, which he had never seen before, and which was quarrelling with his own. No signs of disease were

noticed in the strange dog; it was seen for several hours afterwards to play about, gnaw bones, &c., and was subsequently lost sight of.

The medical man who was called in wished to cauterize the three small wounds with the hot iron, but the patient refused, and his surgeon readily yielded as the dog was supposed to be healthy. Forty days (the period vulgarly believed necessary for incubation) passed away, and the patient now exclaimed that he had been more afraid than hurt, for he had felt uneasy in his mind ever since the accident. About a fortnight after this, the man began to give signs of peevishness, and became quarrelsome and abusive. He soon complained of pain in the vicinity of the pharynx, difficulty of swallowing came on, and the fits of dyspnœa, when water was placed in his mouth, were dreadful.

The surgeon in attendance noticed that the papillæ at the base of the tongue were much enlarged, that the epiglottis became quite erect, and resembled in shape and color a small cherry. Every time that he succeeded in lowering it, the respiration became easier. There was no horror of water, but a difficulty of swallowing it, the fauces, larynx and pharynx being highly injected. The spasmodic fits succeeded each other with fearful rapidity, the cellular tissue of the neck and chest became emphysematous, and the patient died asphyxiated the day after the attack.

No post mortem examination was allowed, but the larynx and posterior portion of the tongue were taken out; they did not present any distinct alterations. M. Guyon appends to the case the following remarks:

1st. It is clear, judging from the case just related, that a dog can communicate the disease whilst the latter is yet latent; and if it be the salivary secretion which conveys the virus, it does not seem necessary that the animal have a great abundance of it, and foam at the mouth, for the fluid to be contagious as is vulgarly supposed. 2nd. It has been imagined that fear and apprehension have much influence on the development of hydrophobia; but this case shows that the disease may manifest itself more than a fortnight *after* every sort of apprehension has passed away. 3d. The peculiar rising and stiffening of the epiglottis noticed in this case, has not been observed before, and would sufficiently explain the laryngeal spasms, which

eventually brought on asphyxia. The phenomena of hydrophobia might thus be accounted for, without having recourse to a lesion of the nervous centres, which lesion has, in fact, never been demonstrated by autopsy. 4th. The emphysema of the neck and chest was probably due to the rupture of some pulmonary vesicles during the violent spasms which the patient had experienced. This circumstance shows very clearly that there must be direct communications between the general subcutaneous cellular tissue and the submucous areolar texture of the lungs. 5th. The hypertrophied papillæ at the base of the tongue, which were seen very early in the course of the disease, would explain the sense of pharyngeal constriction of which the patient complained at the very outset. Thus it would finally appear, that it is principally in the pharynx and larynx that hydrophobia becomes localised, although it is likely that the disease arises from a virulent infection of the whole organism.—*London Lancet.*

Quinine in Continued Fever. By Wm. M. Bolling, M. D., of Montgomery, Ala.—But although I have never myself been able to cut short by the use of Quinine, an unquestionable case of Typhoid Fever, and although it is now, I believe, pretty generally the impression among such physicians of this section of Alabama as I have conversed with on the subject, that it cannot be so arrested, it is more, whatever my own belief, than I would be willing to assert, that it may not be done. I have never myself given the remedy in Typhoid Fever to the extent indeed that Dr. Fearn did, in the cases in which he succeeded with its use, though I have frequently given it in *mild* cases without this effect, in doses, with which I am in the habit daily of arresting with certainty and at once the most *violent* attacks of the various forms of miasmatic fever.—Either then my doses have been too small, or the disease now called Typhoid Fever among us, is different from the cases which were treated with Quinine successfully by Dr. Fearn, and which he calls Typhus, notwithstanding the striking resemblance between them,—and at all events, in both there is this agreement, that in their symptomology they differ widely from any of our recognized shades

of miasmatic fever. Besides the name of Typhus which he gives his cases, Dr. Fearn speaks of the disease as *continued fever.* He does not speak, however, of the post mortem appearances, and notwithstanding the resemblance, I am forced to the belief, that the disease in question was not the one now known among us as Typhoid Fever. I cannot think it possible that this malady, when established in a recognizable form, can be cut short by Quinine. With great partiality for the remedy, and not a little confidence of success, based upon a long and satisfactory use of it in many other diseases, I commenced its administration in Typhoid Fever, and was not less astonished than mortified at the result. Reflecting upon the probable cause of my failure to cut short the disease in its progress, by a remedy from which I expected so much, notwithstanding that I could temporarily control the febrile action, it occurred to me that it might be owing to an inherent tardiness of the reparative process, in the lesion of the intestinal glands. With the view, consequently, of preventing, if possible, this intestinal lesion, by arresting in its incipiency the fever, the *very early* administration of Quinine became a main point in the treatment of the disease with me, till, I think, the experiment was fairly tried.

I have given Quinine in Typhoid Fever, in its various stages, but without observing any difference in its effects, in any way connected with, or growing out of, the period to which the case had extended. I have given it in some cases as soon as the nature of the case was manifest, and n others, at various periods to the termination of the fourth, fifth or sixth week, and still the effect has been the same. I have given it, too, at so early a period, that no one, from the symptoms, could say whether the case was or was not one of Typhoid Fever, and yet the character of the case has been developed and declared after its suspension, and the disease has pursued its ordinary course. Possible exceptions have been mentioned.

As to my doses—in one case I gave twenty grains, repeating it in two hours, and thirty-six grains daily for the two or three succeeding days, in three doses of twelve grains each, at intervals of two hours; always between midnight and day, supposing that the febrile action would be less at this time. In other cases, I have given daily

say two doses of 16 grains each, or three doses of 12 grains each, at short intervals, repeated for several days in succession; and again I have kept up its continuous operation by doses of 8 or 10 grains, repeated every six or eight hours for several days; and always the result has been much the same. Nor have I neglected any adjuvant measures with which I was acquainted, calculated to secure its beneficial influence. I have given it in combination with full doses of Morphia, and I have given it without; I have preceded its administration, where the state of the bowels would admit of it, with small doses of blue mass; I have used the warm foot bath and warm drinks; I have used tepid or cold ablution of the entire surface, and cold drinks.—*N. O. Med. Jour.*

On the Respiration of the Alligator, and Mrs. Willard's Theory of the Circulation. By Albert Welles Ely, A. M., M. D., of New Orleans.—In the case of the alligator, vivisected on the 20th of August, 1851, which made Dr. Cartwright a convert to the new theory, and caused him "to become one of the standard-bearers of the *Filia nata Jovis** of the New World," we doubt whether the alligator was really asphyxiated; for the ligature was not about his trachea more than an hour. Dr. Dowler says about 30 or 40 minutes; Prof. Forshey 20 minutes; and Dr. Cartwright about an hour. Now, how is it possible that the alligator in question could have been asphyxiated, or stone dead, or dead at all, after having a ligature about his trachea for only one hour, at most; when it is well known that alligators can remain under water almost any length of time? They are known to lie at the bottom of lakes and pools of water whole days, without coming out to take the air. How, then, can an alligator be killed by tying a ligature about his trachea? Undoubtedly they become stupid on the application of the ligature, and that they remain so for a time. When they go to the bottom of lakes, they lie there perfectly still and stupid, appa-

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*Mrs. Willard has assuredly too much good sense not to disclaim thi inflated compliment of Dr. C.

rently, until, after a long time, they arouse themselves and come out.

But we have proof positive on this point. We know, positively, that the ligature about an alligator's trachea for one hour only, will not kill him. I will cite a case: On the 6th of May last, the trachea of an alligator was tied by Dr. Dowler, in the presence of others of this this city, for the express purpose of repeating Dr. Cartwright's experiment of inflating the lungs, made on the 20th of May, 1851. The animal became stupid, like the other, at first; but just as they were preparing to inflate, the animal, who appeared dead, suddenly revived *of his own accord*, and fought the by-standers most desperately, *in spite of the wicked ligature about his trachea!!*

What, then, becomes of Dr. Cartwright's statement, that "it is a remarkable fact that tying the trachea is the only means by which that animal can be expeditiously killed?" And what, too, becomes of the proof, afforded by the experiment of the 20th of August, 1851, of Mrs. Willard's theory? Is it not highly probable, that that alligator was no more suffocated to death than its brother martyr to the cause of science of the 6th of May, 1852? Undoubtedly it would have revived, with the ligature about its trachea, if let alone. If Dr. Cartwright had suffered the alligator to remain undisturbed a little longer, he would have been spared the pleasure of recording the apotheosis of Mrs. Willard, and of congratulating her on the probable happy result of her new theory—the discovery of "the art, so long sought by the ancients, of making the old younger, children healthy, men vigorous, and women pretty."*

Dr. Cartwright will please excuse me for my seeming incredulity, for I am never over-disposed to take too much for granted. His Boston opponents, who probably never saw an alligator in their lives, are excusable for not questioning the data of the case.

Dr. Cartwright states, that the heart of the alligator (vivisected on the 20th of August, 1851, by Dr. Dowler,

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*See Dr. Cartwright's letter in the Boston Med. Journal of January 14, 1852.

and upon which alone he bases the "demonstration" of Mrs. Willard's theory) was dead and cold—that he took it in his hand—that not a motion or sign of life occurred. We do not question that he took the heart in his hand, and that it felt cold. He probably knows that the heart of an alligator always feels cold; but as to the heart having ceased to contract, in an alligator like that, just killed, we think there must be some mistake. The heart of the alligator is the last thing to die. It will contract itself for hours after being entirely removed from the body. Neither Dr. Dowler nor Dr. Nutt, who were present, observed that the heart was dead, although Dr. Dowler performed the dissection. These facts, together with another, that Dr. Cartwright did not write his account of the experiments to Mrs. Willard until some months had elapsed, leave room for supposing that there is some mistake about the heart being dead. We think it could not be so. The heart of an alligator, five or six feet long, is small, and contracts slowly and at considerable intervals. If the heart still contracted, in spite of the ligature about the trachea, the circulation must have been going on, and therefore the inflation of the lungs did not start the circulation.

The circulation of the alligator is independent of respiration, as is proved by its being able to remain under water for whole days, and by the experiment of the 6th of May last, in which the alligator, with his trachea tied, revived of himself, after lying stupid about half an hour. The structure, too, of the circulatory system, in the alligator, is precisely such as would lead one to suppose that he could live without breathing, and even with a ligature about his trachea. About one half of the blood in the alligator circulates constantly through his system, without ever going to the lungs at all. His structure is such, that it appears to be optional with him whether his blood be ærated or not. Like all other animals, he can live as long as his blood circulates, and this must take place, whether he is at the bottom of lakes or basking in the sun on their shores.—*N. O. Med. & Surg. Jour.*

The Cause and Prevention of Death from Chloroform.—Dr. Snow said that when dogs, cats, or rabbits were made to breathe air containing from three to five per cent of vapor of chloroform till they died—a process which occupied generally from ten to fifteen minutes—the heart continued to act for a minute or so after the breathing had ceased, as he had ascertained by means of the stethescope, and then, in some instances, the animal gave a few gasping inspirations about the time when the heart was ceasing to act, which had the effect of restoring it to life. On the other hand, when such such animals were made to breathe air containing eight per cent or more of the vapor, death took place very suddenly, the respiration and the heart's action ceasing together. He had indeed performed three experiments in which the action of the heart stopped before the breathing. In experiments with sulphuric ether, the action of the heart always survived the respiration, as air containing fifty per cent of ether vapor was not more powerful than when it contained but five per cent of vapor of chleroform. Ether could, however, be made to act directly on the heart, by continuing to exhibit it by artificial respiration after the natural breathing ceased.

He believed that no accident had occurred from the continued exhibition of chloroform vapor, well diluted with air. In the fatal cases which had happened, death had taken place suddenly, by the way of syncope, showing that the heart had been paralysed by the action of vapor constituting not less than eight or ten per cent of the air inspired just before death. He enumerated all the deaths which he considered to have been caused by the administration of chloroform. They were eighteen in number. In sixteen of these, the agent was exhibited on a handkerchief, or towel, or piece of lint, and in the two cases in which some form of inhaler was employed, it was not used by a medical man. The subjects of these accidents had enjoyed a greater amount of general health than the average of those who had taken chloroform; none of them were children or old people, and the operations which were intended or had been commenced, were, with two or three exceptions, of a trifing nature. He considered the reason of this to be, that under such circumstances the same amount of care was not always employed as in more serious cases. There were two methods of ensuring the

dilution of vapor of chloroform with atmospheric air, to such an extent that death could not occur without giving sufficient warning to allow of accidents being prevented by ordinary attention and skill.

The first and best of these methods was, to exhibit pure chloroform by means of a suitable inhaler; the other method was, to dilute the chloroform with rectified spirit of wine, before pouring it on a handkerchief or sponge. One part, by measure, of chloroform, to two of spirit, which constituted the strong chloric ether of Warren, of America, answered very well; but he (Dr. Snow) gave the preference to equal parts, by measure, of chloroform and spirit, which he was in the habit of applying by means of a sponge, in operation on the face, when he could not employ the inhaler. The best means to be employed in case of impending death from chloroform, was artificial respiration. He believed, from experiments he had performed on animals, that if it were instituted within half a minute of the apparent death of the patient, it would, in the greater number of cases, be attended with success. If this measure did not very quickly restore the patient, it would be advisable to open the external jugular vein, whilst still continuing the artificial breathing, in order to relieve the distension of the right cavities of the heart, which in these cases begins to take place as soon as its action ceases.

Dr. Crisp had found twenty cases of death from chloroform had been published. He had placed them in a tabular form, and enumerated them to the Society. His opinion respecting chloroform, deduced from those cases and his own observation was, that we could not employ it, even to healthy individuals, without some amount of danger. He differed from Dr. Snow in his opinion regarding the impossibility of robberies being effected by the agenby of chloroform, as it might easily be applied to half inebriated persons. An important question was, what are the ultimate effects of chloroform on those who take it in surgical operations? This, he contended, could only be answered by 15,000 or 20,000 cases.

Dr. Theophilus Thompson inquired the experience of Dr. Snow respecting the effects of chloroform on the system in certain cases in which the change effected was often durable or important; in fact, a train of symptoms in-

dicating more or less congestion of the brain, and lasting for many days, or even longer.

Mr. Bullock said that the effects of chloroform were materially modified by its purity; much impure chloroform was manufactured. He had seen it administered to a patient, who was three hours under its influence, without any bad effects.

Mr. Richardson believed that cases of death from chloroform had occurred which had not been alluded to by Dr. Snow, and he mentioned one in particular which had taken place in Bruges. In deaths from chloroform in animals, he found the right side of the heart congested, and left auricle contracted; for thirty-five minutes after death in one case—the peristaltic action of the bowels also continued after death.

Dr. Camps considered that the influence of idiosyncracy should not be overlooked in estimating the effects of chloroform in particular instances. He believed that the first effects of chloroform were principally on the nervous system.

Mr. Barlow thought that the danger of administering chloroform by sprinkling it on a handkerchief, had been overrated. If there were not too much chloroform used, if the handkerchief were not placed too near, and there were not too many bystanders, and the patient were properly watched, he saw no reason to fear the administration of chloroform without an instrument. The proper note of the effects of chloroform during its administration was most essential. One person should always be watching for these bad effects. He had never seen a patient in danger, so long as the iris remained contracted; but if the iris dilated, the inspiration became difficult and the pulse flagged, the chloroform should be immediately discontinued. The deaths in some cases, he believed, had arisen from the desire, on the part of the operator, to administer the agent too rapidly. When death occurred, he believed it was from the heart becoming suddenly affected. The history of cases, however, in which the use of chloroform had been attended with a fatal result, had not, in some cases, been sufficiently stated for us to form a correct opinion upon them; often the previous condition of the patient was not mentioned. In some of the cases better recorded, a defective condition of the structure of the

heart was present. What should we do in a case of poisoning by chloroform? We should employ all the means recommended by Dr. Snow, and more espicially resort to artificial respiration early.

Dr. Sibson said, that owing to the researches of Dr. Snow, we were now in a condition to determine the exact quantity of chloroform which was admitted into the system. He believed that in cases of death from chloroform, it was the heart that was at fault; the symptoms presenting themselves, such as sudden pallor, etc., showed this to be the case. He did not agree with Mr. Barlow, that it was necessary to stop the chloroform when the iris contracted, as in some cases, as of dislocation and hernia, it was necessary to carry the use of the agent beyond this point, which might be done with safety under proper precautions. With respect to the use of chloroform in neuralgia, it was not necessary to carry it to the extent of unconsciousness; anæsthesia was produced before this. With respect to the number of deaths which had occurred from the use of chloroform, he thought this was little, in comparison with the number of persons who had been relieved by its use.

Mr. C. Clark inquired the experience of members in respect to the employment of chloroform in midwifery.

Dr. Murphy advocated its use in midwifery in suitable cases, and with proper precautions. Deaths from chloroform were usually the result of carelessness in its administration—deaths resulting from the employment of too concentrated a dose, which acted suddenly upon the heart.

Dr. Chowne spoke at some length on the importance of the subject, and the circumspection which was necessary to be observed in the employment of chloroform. With respect to its use in midwifery, he thought many disasters had resulted from it, not only as referred to immediate, but to after consequences. Many cases had occurred which had not been published. He cautioned the members respecting the employment of chloroform, to which he was no enemy, but he was desirous to see its use accompanied with the greatest caution.

Dr. Snow said in reply, that the difference between the number of the deaths from chloroform in the list he had given, and in other lists, arose chiefly from the circumstance that he had excluded some deaths which had been

attributed to this agent, but were due, in his opinion, to other causes. The case of the child mentioned by Dr. Crisp, for instance, occurred in Germany, during the excision of a very large nævus on the side of the face and neck, which surgeons, who had seen the case previously, were afraid to meddle with. The operation lasted eighteen minutes, only nine drops of chloroform were applied altogether, and none at all during the last eight minutes. Death was evidently due to syncope from the effects of the operation. As regarded the cases in which death happened during a second attempt to render the patient insensible by chloroform, they could not be attributed to the cumulative effects of the vapor which had been inhaled during the first process; for chloroform could not accumulate for more than twenty or thirty seconds; after this time it began to be exhaled again. These cases clearly illustrated the uncertainty and irregularity of the means which had been employed in administering the chloroform, and showed that the accidents were not due to any peculiar susceptibility to its effects on the part of the patient, who could not have two different idiosyncrasies nearly at the same time; first, a want af susceptibility, and a few minutes or half an hour afterwards, a greater susceptibility than usual.

In reply to Dr. Theophilus Thompson, Dr. Snow stated, that he had not met with any unpleasant sequalæ which he considered to be the effects of chloroform, except sickness, which had in a few cases been troublesome for two or three days, and hysteria, the latter of which might certainly occur from an operation without chloroform. If depression existed from the long-continued administration of chloroform, it should be removed by warmth and cordials.—*London Lancet.*

Improvements in Modern Surgery.—Dr. Mussey in his last *Introductory Lecture*, enumerates some of the chief improvements made in surgery during the present century. He says:

In the healing of *simple wounds*, water dressings may often be so managed as to effect an entire union of the divided parts without suppuration, and this, too, where

the divided parts are not kept in contact, as is necessary for the adhesive process, the space being filled up by the requisite vital materials without suppuration. This has been called the modeling process, by Dr. Macartney.

Certain poisoned wounds: the bite of the rattlesnake, it has been alleged on pretty good authority, has been cured by the early local application of tincture of iodine.

Tetanus (traumatic): a number of well authenticated cases have been successfully treated by chloroform.

Erysipelas: locally, iodine, nitrate of silver; internally, carbonate of ammonia, opium, and quinine. In phlegmoid erysipelas, as well as carbuncle, incisions are of great value.

Anæsthtic agent: a priceless boon, recently handed down to us by a kind Providence to disarm surgery of its terrors. In my practice ether and chloroform have been used without injury, in over six hundred operations. In St. Bartholomew's Hospital, in London, according to Mr. Skey, in a statement made last summer, chloroform had been employed, without injury, in between nine thousand and ten thousand operations. The French Academy awarded for this discovery, to our countrymen, Doctors Jackson and Morton, each a medal; the one for having made the suggestion, the other for having put the suggestion to experiment.

In surgical anotomy, great process has been made. This is exemplified in the anatomy of the neck, in reference to the extraction of tumors and the ligating of vessels. The anatomy of hernia has arrived at wonderful precision, compared with what was known a century ago. Various accompaniments of strangulated hernia—difficulties to be encountered, and means of meeting them—are far more clearly understood.

Surgery of the Arteries. This department has undergone such important modern improvements, as almost to admit of its origin being dated about the time of Hunter. Ligating an artery on the cardiac side of aneurismal tumor has been chiefly practiced. The artery, too, has been successfully ligated on the distal side of the tumor, as recommended by Brasdor. Success, too, has repeatedly followed the compression of the artery on the cardiac side of the tumor; and even on the distal side, as reported by Dr. Goldsmith, of Vermont. In a recent aneurism,

according to Dr. Dudley, a compress upon the tumor, supported by a bandage upon the whole limb, has effected a cure.

In 1845, the subclavian artery had been litigated *sixty-nine times:* of this number there were thirty-six recoveries and thirty-three deaths.

Dupuytren and Liston, each has tied successfully the subclavian artery, under cover of the anterior scalenus muscle.

The aorta has been ligated *four* times, the arteria innominata *nine* times, and the subclavian, on the tracheal side of the scaleni muscles, *five* times. Death followed in every case, leaving surgeons little room to hope for any better result from a repetition of either of these operations.

The surgery of the eye has been greatly improved by Scarpa, Maunoir, Adams, Lawrence, Mackenzie, Tyrrel, Alexander, Roux, Velpeau, Sichel, Desmarses, Cuvier, Rogers, Delafield, Pancoast, Hays, and a host of others.

In the surgery of the ear, Itard, Deleau and Kramer, have been distinguished.

Plastic surgery has undergone important modern improvements. Taliacotius, of Bologna, in Italy, in the latter part of the sixteenth century, gained a temporary celebrity by his rhinoplastic operations, in which he formed a new nose by a flap of skin taken from the arm. This operation fell into disuse for a long period. Rhinoplasty, however, was revived and improved by Messrs. Carpue and Linn, of London. More recently, plastic surgery attracted the attention of numerous surgeons, as Græfe, Diefenbach, Frick, Zeis, Chelius, Delpech, Dupuytren, Lisfane, Jobert, Liston, J. M. Warren, Pancoast, Mutter, Post and Hullhen.

Staphyloraphy or palate suture was practiced with success by Roux, Græfe, Diefenbach, Warren, and others. This operation has been recently improved by Mr. Ferguson, of London, by dividing the levator palati and palato pharyngeal muscles.

In harelip operations, important improvement has been made, especially in those revolting cases with a double fissure and peninsular portion of the upper jaw connected with the septum of the nostrils, and projecting horizontally forward.

Tenotomy and Myotomy. Great progress has been

made in the relief of deformities, as of club-foot, wry neck, strabismus, permanently contracted fingers.

In chronic enlargement of the tonsils, excision is now employed instead of ligation, which was used forty years ago.

Cancer of the tongue: nearly half of that organ has been successfully removed.

Excision of the upper and lower jaw: the latter operation has been performed by disarticulating the bone without dividing the facial nerve, or the duct of Steno, thus preserving the symmetry of the face.

In chronic abscess of bone, limbs are now saved by perforating the wall of the bone, which were formerly doomed to amputation.

In fractures of the limbs, the wet bandage of Dudley, in addition to splints, should not be overlooked, as marking improvement in the treatment.

In compound fractures, the collodion is of great value in excluding the air from the wound. In un-united fracture, or false joint, the seton, sawing the ends of the bones, subcutaneous scarification, lateral or longitudinal compression of the fragments, ivory pegs and wire bridles, and more especially a suitable constitutional treatment, have, in their turn, promoted cure.

In dislocations, anæthetic agents will probably take the place of copious bleedings, nauseating doses and the extreme warm bath.

In amputations the flap operation, as well as amputations at the hip, shoulder and ankle joints, show a progress in this department of surgery.

Diseases of the joint are better understood than formerly, and the treatment is more successful. Excision of the diseased elbow joint has been repeatedly successful.

Iodine injections in hydrarthus, in enlarged bursæ mucosæ, in ganglions, in spina bifida and chronic hydrocephalus, have been practiced with important benefit.

Injuries and diseases of the brain and spinal cord are much better understood.

THE WESTERN JOURNAL.

THIRD SERIES.—VOL. X.—NO. II.

LOUISVILLE, AUGUST 1, 1852.

HEALTH OF LOUISVILLE.

The diseases of earlier summer have abated. We hear now of but few cases of bowel complaints. Bilious remittent fever is becoming somewhat prevalent in the suburbs of the city, and with it cases of intermittent fever are occurring, but in the interior nothing is heard of these diseases. Cholera is not spoken of in any part of the city.

The summer is again one of the hottest; it is indeed seldom that we have two such summers in succession. For a number of days towards the latter end of July, the thermometer ranged from 93° to 96° in a fair exposure.

CHOLERA.

This is the fourth year since cholera made its second appearance upon our continent, having broken out about the same time in New York and New Orleans, in the winter of 1848–'9. The epidemic hitherto has conformed in a remarkable manner to the course which it pursued in its former progress, and if it should continue to do so, this is the last summer that it may be expected. It appeared before in Canada, in 1832, invaded the Eastern States, and spread as far as Cincinnati, St. Louis, Louisville and Lexington in the autumn of that year. The next year its fatal ravages were felt in Kentucky and Tennessee; it lingered

until 1835, during which it visited Versailles and some other towns in Kentucky and Tennessee with great violence, and then disappeared. Many of the places which suffered in the former epidemic have thus far escaped, and some which were exempt before have suffered with the present plague. At this time we hear of its irruption in numerous localities where it was prevalent in 1833. During all the season it has appeared, from time to time, in one town and another, generally prevailing for a few days only, and with but slight mortality. The force of the pestilence would seem to be spent. It nowhere manifests the malignity which characterized its first appearance. It is not epidemic in any of our great cities, at the same time that most if not all of them have afforded occasional cases of cholera. There appears to be no tendency to an epidemic. Two or three inmates of a family are attacked and carried off, and there it stops. In this city all through the year, we have had such cases, now and then, but although dysentery has been epidemic and cholera morbus and diarrhea have been unusually prevalent, these complaints have rarely if ever run into cholera; from all which we infer that the epidemic tendency has ceased.

CIRCULAR.

We publish below the Circular of the Committee *on the Epidemics of Tennessee and Kentucky*, which we hope will awaken a due interest in the physicians of those States. The chairman of the committee informs us that although the profession was not well prepared to respond to the call of the American Medical Association when made a year ago, still a number of physicians set zealously to work and made out valuable reports. The circumstances are now more favorable. The same committee is to be continued for five years, and those who could not at once make a report will be able to do so at their leisure. But all who have any desire or ambition to aid in the advancement of their profession by investigating the epidemics passing under their observation, should go to work immediately, and collect, and arrange the statistics asked for by the committee. The enterprise is a highly important one, and it is to be expected that our brethren generally will be disposed to expend time and labor in its furtherance.

GEORGETOWN, July 26, 1852.

To the Editors of the Western Journal of Medicine and Surgery:

Gentlemen—I send herewith a call upon our brethren for materials

for compiling a history of the Epidemics of Kentucky and Tennessee. Will you be so good as to allow it a place in the forthcoming number of your Journal. I hope, too, that you will be able to allow room to the accompanying table. I look upon it as a very important item in the information sought. With all the disadvantages of last year I was very much gratified at the number of cases tabled. There were about 400 of dysentery alone. Under more favorable circumstances I hope we may do still better. I think the table at the end of each disease treated of gives a finished appearance to the job. Besides, it is an example set of giving the relative mortality of each disease, which I hope will be followed.

Very truly yours,

W. L. SUTTON.

TO THE MEDICAL PROFESSION IN TENNESSEE AND KENTUCKY.

It is known to you that the American Medical Association is anxious to be supplied with correct and reliable histories of Epidemics prevailing in different portions of the Union. For this purpose it has divided the country into districts and appointed Committees to investigate and report on the epidemics of each. To enable these committees to discharge their duties to the best advantage, they are to continue for five years.

It will be seen at once, that if the physicians of the United States will set to work, record faithfully their experience, and at the end of each year transmit to the appropriate committee the result of their observations, we shall have a history of Epidemics greatly more reliable and valuable, and embracing a range of country vastly more extensive than our profession has ever seen.

Is it necessary to argue this point? Is it needful to say that this great object can only be accomplished through contributions from many individuals? Is it requisite to say to each individual physician, that he is personally called upon to furnish any facts which may be within his knowledge in aid of this great enterprise? It is hoped not.

The committee appointed for the States of Tennessee and Kentucky earnestly desire *carefully observed facts* upon any subject connected with Epidemics; but would suggest *particular* attention to the following heads:

Causes supposed to have given rise to the Epidemic.

Causes which favored its spread.

Causes which retarded its progress.

Prophylactics.

Age, sex, color, employment, diet, and habits as to exposure and temperance of those most liable.

Time and extent of its prevalence.

Prominent symptoms during its different stages.

Proportional mortality—*Post mortem appearances.*

Treatment.

Medical topography, including the nature and geological characters of the soil—character and average temperature of the springs and wells.

Meteorological observations—mean monthly and annual temperature, weight and moisture of the atmosphere, &c., &c.

It is considered important that as many cases treated should be reduced to tabular form as possible—a form for which is annexed—and returned with the reports.

As the members of the committees are widely separated, to enable them to consult each other, it is very important that reports be made by the 15th July annually to some one of them.

Wm. L. Sutton, M. D., Georgetown, Ky.
Asbury Evans, M. D., Covington, "
Frank A. Ramsey, M. D., Knoxville, Tenn.
Thomas Lipscomb, M. D., Shelbyville, "
E. B. Haskins, M. D., Clarksville, "
} *Committee.*

		NO. ATTENDED.							NO. DIED.							
Diseases.	Col'r.	Under 15		15 to 40.		Over 40.		Total.	Under 15		15 to 40.		Over 40.		Total.	Avr. time of Attend.
		M.	F.	M.	F.	M.	F.		M	F.	M.	F.	M.	F		
Cholera,	W															
	B.															
Cholera Infantum,	W.															
	B.															
Diarrhea,	W.															
	B.															
Dysentery.	W.															
	B.															
Erysipelas,	W.															
	B.															
Fever, Bilious	W.															
	B.															
Typhoid	W.															
	B.															
Scarlet,	W.															
	B.															
Hooping cough,	W.															
	B.															
Influenza,	W.															
	B.															
Measles,	W.															
	B.															
Small-pox,	W.															
	B.															
	W.															
	B.															
	W.															
	B.															

State how many of the above cases were seen secondarily,—in consultation or otherwise.

DR. GRIMES' ADDRESS.

Dr. Samuel Grimes, of Delphi, Ind., delivered an excellent address before the Indiana Medical Convention, at New Albany, in May last, which has been published. We had the pleasure of listening to the address, and have since read it with much interest. We would be glad to copy it entire, but our limits will admit of only a few extracts.

"Before we blame with asperity the wrong bias of a portion of the community towards medical heresies and quackery, we, of the profession, ought to have the consciousness that we are clearly distinguishable from the whole tribe of pretenders, by our superior attainments, not only in medicine, but in general literature and science, as well as by our greater readiness of resource at a time of difficulty and danger in the sick room. The mere fact of our holding a diploma in Latin, while others put up with one in English, and of our believing in one doctrine, while they are clamorous in favor of another, will give us no valid claim on the world for higher professional rank, and greater skill in *practice*. We must rest our claims on something more substantive and appreciable than this. We may dwell with allowable pride on the antiquity, and learning, and signal philanthropy of the great family, with Hippocrates at its head, to which we *belong*. But the pride of ancestry becomes ridiculous if it is not sustained by contemporary merit. To be received in the great brotherhood and decorated with the order of merit, we ought to be able to show that we underwent early preparation by a good academic or collegiate education; and early training afterwards, by elementary medical instruction, in the office of an experienced physician, before we were enrolled and became regular and faithful attendants in a Medical School of repute and competent organization.

"Some, perhaps many of us, will confess, with regret, that we have not been able to comply with these requisites, in all their particulars. But, surely, this does not imply that we should withhold our advice and assistance, in order to remove the obstacles from the path of those coming after us, which embarrassed and impeded our own course. The times are more propitious to the zealous student now than they were in our early days; and, consequently, there is less excuse now than then for neglect, on the part of a parent, to procure for his son a good preparatory education, and on that of a physician to impart to his pupil suitable elementary instruction in medicine. Nor need any of us stand excused from the discharge of his duty to his pupil, if he received any such in his office, by the plea of his own defective attainments. However limited may be our reading and knowledge of medical literature, we can still point out to the young student a good book on each of the different branches of medicine; and early familiarize him with the qualities of drugs, and, to a certain extent, their pharmaceutical combinations.

"In pursuing the course here recommended, we shall discharge not only our duty to our students, but, also, to the several Medical Colleges to

which they will resort after leaving us, for on the proper elementary instruction which these young men have received at our hands, will mainly depend their successful indoctrination in the higher principles and rational practice of Medicine by the professors in the schools. These gentletlemen are continually embarrassed in their teachings by not knowing what foundation has been laid by the private teachers, or by those who ought to have been such; nor how far they may take for granted an acquaintance with elementary truths on the part of the youthful auditors. One professor, for fear of going beyond the comprehension of his class, contents himself with uttering the merest *common places;* another aims to bring up his hearers to his own elevated standard. If it be said that the first is the most popular lecturer, does not the fact show the unpreparedness of the students for a course of instruction such as they ought to receive, and such as an able and conscientious professor ought to be able to give.

"The dignity, learning, elevated standing, and successful teachings of the Faculties of our Medical Colleges cannot be matters of indifference to us. They are, in one sense, our representatives, in another our auxiliaries; and whatever affects their reputation must re-act on the entire body of the profession. We cannot, therefore, be supposed to look with indifference on the organization of these colleges, nor be expected to give any countenance to those with the entire competency of whose Faculties we are not fully satisfied. A new Medical School cannot, like a new store, or a new manufactory, or a new line of stages, be patronized, merely because it is new, and to encourage trade and reduce prices, by competition. Nor can personal regards nor sectional pride furnish an apology for our sending our students, or otherwise giving our countenance to a school, formed in haste out of incongruous materials, and as if in a spirit of mere business speculation. The medical profession, at large, must divide the responsibility with the Medical Schools for the attainment and fitness of the graduates sent out annually from these latter, as candidates for practice and public patronage. If we send to the Medical Colleges crude materials, it cannot be expected that they should be at once fashioned into shape and harmonious proportions. If we send to them ignorant minds, we have no right to suppose that these will be instructed and taught the principles of medical logic, and philosophy in the course of a few months.

"I repeat, that we have a large share of responsibility for the manner in which Medical education is conducted. We send our students to be educated, we influence them in their selection of the school to which they go. We, therefore, mistake greatly our position, if we think that we can assume the office of judges and critics of Medical Schools without our participating in their cares and difficulties, and giving our counsel for their protection and guidance, when they come up to what we believe to be a good standard of teaching. The best assistance we can give them, is to send them students properly prepared to avail themselves of the higher course of instruction which the professors are ready to go

through with. The best counsel we can offer to the professors, is for them to abide by their own convictions of duty in keeping up an elevated standard of medical attainments for their students, and taking adequate time for imparting their knowedge to these latter. Not only are there more branches of medicine to be taught now, but each branch is much richer in details, than formerly; and hence it is clear that more time is required to teach them than formerly was requisite.

"It may be asked, whether we are ourselves, either by education or subsequent reading, competent judges, in the premises, of what is necessary on the score of Medical Education? It may be that many of us are not, in this sense *competent*, but the very consciousness of deficiencies, and experience of what we have suffered in consequence may naturally impel us to seek reform, and create a desire that others coming after us should enjoy advantages of which we were deprived.

"We may, however, all of us, in this age of periodical literature and abundant publications, readily procure the desired information on many subjects of immediate interest. The calls on our time and attention in the cares of practice, and especially country practice, must always prevent our being regular and systematic readers: but if we are bent on turning all the odd hours to account, and have a good medical journal, and the best monograph at our elbow, we shall be able to keep up, to a respectable extent, with the progress of our science, at least so far as to appreciate its extent and chief bearings, and the improvements in practice. In this way we can do justice to ourselves and to our students, and confer, if need be, understandingly with our brethren, the members of the Faculties of Medical Colleges."

ENCOURAGEMENT OF QUACKERY IN GEORGIA.

The *Southern Medical and Surgical Journal* says: "Georgia is, we believe, the only State in the Union, that has drawn upon its treasury for the encouragement of quackery. The last session of her Legislature appropriated five thousand dollars to the Thompsonian or Botanic School, located at Macon."

This is humiliating. It is a premium to ignorance—an outrage upon science, and, in the estimation of educated men all over the world, disreputable to the Commonwealth. Let quackery have its course; leave it to the people, to adopt or reject it; but for statesmen—framers of laws, to go about patronizing such a system as Botanic Medicine, is simply disgraceful.

NEW MEDICAL BOOKS.

We have received a number of new medical works which we shall be able to notice at length at an early day. The publishers, Blanchard & Lea, have issued a new edition of *Dr. Bartlett's Treatise on the Fevers of the United States*. We are often asked by students of medicine for the best work on this subject, and have no hesitation in referring them to this. They will be glad to learn that a new edition has been prepared by its accomplished author, and appears in the best style of its enterprising publishe s.

A new edition of *Wood's Practice* has also been issued, of which a review was prepared for the present number but was excluded by the press of matter on hand. It will appear in our next number.

Messrs. Blanchard & Lea have laid the profession under further obligations by new editions of *Meig's Treatise on Obstetrics*, and *Miller's Principles of Surgery*, both works of great ability, which have acquired a wide and enduring popularity.

The same publishers have issued the *Surgical Lectures of Bransby B. Cooper* in a style corresponding to the other publication of their house. It forms a handsome octavo volume of 759 pages, with an ample index, and may be confidently recommended to students and practitioners as a storehouse of reliable information on all Surgical topics.

Analysis of Physiology is the title of a small volume from the pen of *Dr. John J. Reese*, for which we are indebted to Messrs. Lindsay & Blakiston. It is designed to facilitate the study of this most pleasing branch of medical science, and is sure to find many purchasers among the numerous medical students who, in a few months, will be crowding the various schools of the country.

Dr. Horace Green, of New York, has made a valuable contribution to our professional literature in his little volume on the *Surgical Treatment of Polypi of the Larynx and Oedema of the Glottis*, a copy of which we have received through the publisher, G. P. Putnam, and which we shall take the earliest occasion to notice in detail.

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ART. I.—*Observations on Excision of the Superior Maxillary Bone; Illustrated by seven Cases.* By S. D. GROSS, M. D., Professor of Surgery in the Medical Department of the University of Louisville.

The diseases of the superior maxillary bone, requiring the operation of excision, and generally described under the vague and unmeaning name of *osteo-sarcoma*, constitute a numerous and important group of affections, defying every attempt at correct classification. That this declaration is not a mere assumption, unsupported by facts, the writings of pathologists and surgeons abundantly attest. Much as has been written upon this subject within the last quarter of a century, there is still room for observation, both as it respects the nature, diagnosis, and treatment of these maladies. More light must be elicited before this branch of surgery can be placed upon a sure and solid foundation in relation to the propriety or impropri-

ty of the operation of excision in any particular case, or in any particular class of diseases.

It is not my design, in this paper, to write a learned and elaborate article upon these affections; on the contrary, all I shall aim to do is, to point out a few of the more common and important, those which I have most frequently witnessed in practice, and those with which I am, consequently, most familiar from personal observation.

1.—By far the most frequent disease of the upper jaw is *encephaloid*, osteocephaloma, or soft cancer, which occurs here, as elsewhere, in both sexes, in all classes of individuals, and at all periods of life. I have seen it in young children under five years, at middle life, in old age, and in decrepitude. I am satisfied, however, that it is most frequent between the twentieth and fortieth years. In one of my patients it occurred at the age of sixty-six.

It is not known what influence, if any, is exerted upon the development of this disease by *occupation*, temperament, climate, and other circumstances. In every instance of it that has fallen under my observation, the tumor sprang up without any assignable, or palpable cause. Cases have been recorded in which it was apparently awakened by external injury, as a blow, the pressure of the pullikin, or fracture of the bone.

Encephaloid usually *begins* in the cavity of the antrum of Highmore, in connection with the mucous lining, or internal periosteum. Occasionally it takes its rise in the cancellated structure of the bone, in the socket of one of the teeth, in the gum, or in the periosteum, properly so called. In the first case, it generally progresses, with more or less rapidity, until it fills up the whole sinus, after which it encroaches upon the bony parietes of the cavity, pushing them out in every direction, and thereby pressing them against the surrounding structures. As the external wall is extremely thin, in fact, a mere shell, in the natural state, the morbid growth generally advances

more rapidly in this direction than in any other, forming thus frequently, even at an early stage, quite a large tumor beneath the integuments of the cheek. By and by, as it proceeds in its development, it extends inwards towards the nostril, partially, sometimes wholly, occluding the corresponding cavity; upwards toward the floor of the orbit, compressing, and ultimately protruding the ball of the eye; downwards towards the roof of the mouth, displacing the tongue, and diminishing the buccal cavity; and backwards towards the fauces, impeding mastication, deglutition, speech, and respiration. At this stage of the disease, the countenance is most hideously disfigured, and the patient is an object well calculated to excite the deepest commiseration.

The integuments and mucous membrane are generally sound in the *earlier stages* of the malady; but after a certain period, varying from several months to a year or more, they begin to assume a livid hue, become deeply congested at one or more points, and at length yield to ulcerative action. The consequence is, a fungating and rapidly spreading sore, the seat of a thin, sanious, mucopurulent, or sanguinolent discharge, very abundant, excessively fœtid, and highly irritating in its character. Pure blood often proceeds from it; sometimes very small in quantity, at other times so copious as rapidly to undermine the strength, and bring on hectic irritation and exhausting night-sweats.

In the *latter stages* of the disease, sometimes before, but generally not until after ulceration has set in, the lymphatic glands of the temple, behind the ear, and under the jaw become enlarged and contaminated, and finally burst from over distention of their contents. The countenance assumes a peculiar cadaverous hue and expression; the patient rapidly loses flesh and strength; colliquative diarrhea and sweats supervene; the pain is excessive; and death, now a welcome visiter, closes the scene of agony and strife.

The *pain* attending this affection has no fixed or definite character. In general, it is dull and aching, not unlike the tooth-ache, to which, in fact, it is usually at first referred by the patient. As the tumor augments in volume, and encroaches upon the surrounding structures, the pain commonly becomes more severe and constant, and assumes a sharp, lancinating character. It is usually most violent at night, and in damp, cloudy states of the atmosphere; it is also greatly influenced by the condition of the stomach and bowels, by mental emotion, and by whatever has a tendency to produce derangement of the secretions. In some cases the pain is tensive and pulsatile, especially when the tumor is very large, or is about to become the seat of suppuration. Occasionally, again, it is of a neuralgic character, having, like an intermittent fever, regular paroxysms and intermissions. It is rarely limited to the diseased mass, but extends through the neighboring parts, and sometimes even to a considerable distance.

The *progress* of the tumor is variable; sometimes very rapid, at other times quite tardy. In 1844, a boy, five years of age, was brought to me for an affection of this kind, which caused his death in less than six months from its commencement. The tumor was of extraordinary volume, filling the mouth, throat, and nostrils, and causing the most frightful deformity. A short time before death, it gave way in the mouth, fungating and bleeding so profusely as rapidly to undermine the foundations of life.

A case of still more rapid termination was under my observation last summer, in a colored man, aged twenty-seven, belonging to a Mr. Lewell, of Palmyra, Tennessee. The disease, occupying the right maxillary bone, was first noticed in April, 1851, and by the 2nd of July, when the man arrived in town, it had already attained an enormous bulk. The tumor formed a large prominence on the cheek, and almost filled the mouth, rendering it very difficult for the patient to swallow, and to articulate so as to make himself

intelligible. It rapidly increased in size, extending behind the ear, to the eye and temple, beyond the median line, and below the inferior maxillary bone, throwing the chin several inches over to the opposite side. In a few weeks after his arrival, a small slough formed upon the cheek, over the most prominent portion of the tumor, evidently from the effects of the pressure of the morbid mass, and this was soon followed by the appearance of a large fungus, which was the seat of a constant sanious and most loathsome discharge, attended, occasionally, with copious hemorrhage, and the most violent pain imaginable. Under the influence of this accumulation of evils, his general health, already much impaired by previous suffering, rapidly gave way, and on the 9th of August he died. Upon inspection, the tumor was found to be composed of a soft, fluctuating, brain-like substance, the true character of which it was impossible to mistake.

In this case, no one could, of course, for a moment, entertain the question of an operation; the period for such a procedure, if proper at any time, had already passed when the man reached Louisville; and the only reason why he was not sent back, at once, was that he was unable to bear the fatigues of the journey.

On the other hand, I have seen several cases in which the fatal event was delayed for several years. My experience is that the malady is usually more rapid here, as in other parts of the body, in children and youth than in the middle-aged and old.

When such a tumor is *examined*, after removal, it is found to exhibit all the characteristic features of encephaloid formations. That portion which occupies the antrum is commonly quite soft and pulpy, resembling, at least faintly, a section of the brain, both in its color and consistence. The osseous structure is generally broken down and disorganized, quite vascular, and so soft as to be easily cut with the knife. In some specimens it is en-

tirely, or nearly entirely, absorbed; while, in others, it is replaced by fibro-cartilage, or cartilage, intermixed with spicula and scales, remnants of the original substance. In nearly all cases, the morbid mass is remarkably vascular, being pervaded, in every direction, by large vessels, the walls of which are exceedingly brittle, and therefore liable to yield under the slightest impulse. It is owing to this circumstance that these tumors frequently attain such an enormous volume, and that, when ulceration sets in, they are so liable to fungate and bleed.

The *recognition* of encephaloid disease of the superior jaw is usually not difficult. The rapid growth of the tumor, its steady encroachment upon the adjacent parts, its soft and elastic feel, the livid aspect of its buccal portion, and the sharp darting pains which attend it, readily distinguish it from all other formations of this division of the skeleton. In the latter stages of the affection, the fungous character of the ulcer, and the sanious, sanguinolent, and bloody discharges, together with the sallow and cadaverous state of the countenance, and the enlargement of the neighboring lymphatic ganglions, leave no doubt about its nature. Important information will also be furnished by the history of the case, and by the fact that encephaloid occurs at all periods of life, while some of the other maladies of this region are met with only at certain ages.

Where any doubt exists respecting the character of the tumor, no objection lies against the use of the exploring needle, which will at once inform us as to the consistence of the tumor, and the nature of its contents. If it is encysted in its character, an escape of serous, or muco-sanguineous fluid, will afford the necessary intelligence, and enable us to arrive at a pretty satisfactory conclusion in regard to the proper mode of procedure. Should encephaloid matter be present, the smallest quantity, in fact the merest particle, will, if subjected to the microscope, reveal the characteristic cancer-cell. I consider it of great

importance that the exploring instrument should be as fine as possible, that the puncture which it makes may heal by the first intention, and not permit the protrusion of any of the contents of the tumor. There is much danger in malignant diseases, where this precaution is omitted, of doing harm by inducing premature ulceration, fungation, and hemorrhage, followed by the rapid destruction of the patient. I much fear that surgeons have paid too little attention to this circumstance.

Encephaloid of the jaw seldom *co-exists* with malignant disease in other parts of the body. The affection, in fact, in the great majority of instances, is more habitually local than when it invades the cellular tissue, eye, and glandular organs. It is, doubtless, owing to this circumstance that excision of the disease, especially in its early stage, is occasionally followed by a successful result; though, in general, as was before intimated, the prognosis is most unfavorable.

2.—The tumor next in point of frequency is the *fibro-cartilaginous*, or, as is it not uncommonly, but incorrectly termed, *sarcomatous*. It is not easy to describe this variety of growth, so diversified and multiform are its component elements. Its most ordinary form is the fibrous, in which, as the name implies, the fibrous structure predominates, though there is often, if, indeed, not constantly, an intermixture of other elements, especially the cartilaginous; small cysts, containing a thin serous, glairy, or sanguineous fluid, are sometimes interspersed through these tumors. Few vessels are apparent in their structure, and hence they seldom attain a very great bulk, or advance with much rapidity. For the same reason they do not bleed much when ulceration takes place, or during attempts at extirpation.

The original site of this tumor, like that of the encephaloid, is usually the antrum; but it also sometimes begins in the proper substance of the bone, in the socket of one

of the teeth, usually in that of one of the grinders, in the periosteum, or between the periosteum and the outer surface of the bone. However this may be, the substance of the bone is gradually broken down and disintegrated, and ultimately lost in the new deposit.

The fibro-cartilaginous tumor of the upper jaw, notwithstanding that it is not, in general, very vascular, occasionally attains a very large size. Instances have been reported in which it was as big as a fœtal head. Usually, however, it is small, not exceeding the volume of an orange, or an ordinary fist. One of the most interesting facts connected with this tumor, is its non-malignant character, or its tendency not to return after extirpation.

The fibro-cartilaginous tumor can generally be *distinguished* from encephaloid and other formations of the upper jaw, first, by the slowness of its growth; secondly, by its globular, ovoidal, or botryoidal shape; thirdly, by its circumscribed character, or indisposition to ramify through the surrounding parts; fourthly, by its firm and unyielding feel; fifthly, by its painlessness; and sixthly, by the absence of contamination of the neighboring lymphatic glands. The tumor manifests little or no tendency to ulcerate, and the mucous-membrane of the mouth is of a more florid color than in encephaloid. There is also less serous and bloody discharge. The general health is not deteriorated, and the countenance is free from the peculiar sallow and dejected expression which forms so remarkable and characteristic a feature in malignant disease. The fibro-cartilaginous tumor is not confined to any particular period of life, but is most common in young adults.

3.—The *epuliform* tumor is occasionally seen upon the upper jaw, though its favorite site is the lower. This is a sarcomatous, or fibrous growth, of a soft spongoid consistence, and of a red florid complexion, springing originally from the gum, or the lining membrane of the

socket of a tooth, and gradually extending to the bony texture, which, in time, is partially absorbed, or expanded into a soft, elastic, parchment-like shell. By and by, the teeth loosen and drop out; the morbid mass ulcerates and fungates; and the patient is constantly annoyed by a sanious, or bloody discharge, so offensive as to be loathsome both to himself and to his neighbors. The tumor seldom extends beyond the alveolar process; but occasionally it forces its way, even at an early period, into the maxillary sinus, rapidly filling it up, and then protruding its walls in every direction, as in the true osteocephalomatous formation. A decayed tooth, a blow upon the jaw, or injury upon the gum, are the most common causes of this affection. Not unfrequently, however, it comes on spontaneously, under the influence, apparently, of some constitutional taint.

The *diagnosis* of this form of tumor rarely offers any difficulty. Its exposed situation, its connection with the teeth and alveolar process, its want of sensibility, its florid appearance and spongoid consistence, are almost pathognomonic of the nature of the affection. It is only in the more advanced stages of the malady, when the body of the jaw has become extensively involved, when the surface of the tumor has begun to ulcerate and sprout, and the discharge is copious, bloody, and offensive, that the discrimination will be likely to prove embarrassing. The tumor, under such circumstances, might be easily mistaken for medullary sarcoma; a circumstance, however, which need not be particularly regretted, inasmuch as the curative measures are the same in both cases.

The *progress* of this morbid growth is sometimes remarkably rapid; at other times, slow and gradual. As it increases in volume, it must necessarily encroach very materially upon the mouth, thereby interfering with the tongue, teeth, and fauces. Its character is usually malignant, and hence, unless it is thoroughly extirpated, it is almost certain to return after removal. Like encepha-

loid, it rarely co-exists with malignant disease in other parts of the body. Indeed, it would seem, even in its worst forms, to partake more of the cancroid than of the true cancerous structure and habit.

4.—The *sero-cystic* tumor of the upper jaw is uncommon, and, fortunately, devoid of malignancy. Its usual site is the alveolar process, where it has been known to attain the volume of a hen's egg, and even of a large orange. It is composed of a thin, semi-transparent, or slightly opake bag, occupied by a colorless, or sanguineous fluid, or by a glairy, mucilaginous substance. Sometimes, though rarely, there are two such cysts, either closely connected together, or seperated by a kind of osseous septum. The bone around the tumor is expanded into a thin, elastic, crackling, parchment-like shell, and is easily penetrated by a sharp instrument, the puncture giving vent to the characteristic contents of the cyst. This, in fact, is the best diagnostic sign of the morbid growth. The general health remains unaffected in this disease, which is always tardy in its progress, and manifests no disposition to extend among the adjacent structures. Where any doubt exists as to the real nature of the case, a resort to the exploring needle will usually at once dispel it.

It is not often that this tumor calls for removal of the affected bone; in general, it will suffice to puncture it occasionally with a small trocar to evacuate its contents, the escape of which is often followed by the rapid contraction and final obliteration of the sac. Where there is a strong tendency to reaccumulation, a larger opening should be made, and tincture of iodine thrown in, to promote the object in view. It is only in old and intractable cases that excision of the affected bone will be likely to be necessary.

5.—Excision of the superior maxillary bone is sometimes required on account of the presence of an *exostosis*. The tumor, varying infinitely in regard to its size and

form, is most common in young and middle aged subjects; it may appear upon any part of the bone, and gradually augmenting in volume, may at length involve it in its entire extent. It is a strictly local affection, the result often of external violence; and rarely, if ever, degenerates into malignant action. An exostosis is a true bone, growing, as the name implies, from bone, and composed of precisely the same antomical, chemical, and microscopical characters.

An exostosis is easily *recognized*. Its chief peculiarities are, its excessive hardness, its slow growth, its freedom from pain, the absence of disease in the surrounding structures, and the unimpaired state of the general health. There is no discharge of blood, or muco-purulent matter, no tendency to ulceration, no alteration in the skin of the face, or mucous membrane; the principal inconvenience which the patient experiences is from the size of the morbid growth, which is occasionally very considerable, and from its consequent interference with the functions of the adjacent parts. When doubt exists in regard to the real character of the disease, a small exploring needle, introduced at various points of the swelling, will at once decide the question.

6. Under the same head with exostosis may be mentioned an enlarged state of the bone, constituting a species of *hypertrophy*. An example of this variety of morbid action will be found in the first case appended to this paper, in which the tumor, occurring in a young lady of nineteen years, was occasioned by the irritation of an inverted, or misplaced canine tooth. The bone was expanded into a thin shell, about the volume of a large hickory nut, and was of a hard, firm consistence, with but little pain, and no constitutional derangement. The tumor was evidently of local origin, and the operation, performed in April, 1845, was perfectly successful; the part remaining well up to the present moment.

Mr. Stanley* mentions an instance in which the hypertrophy involved the whole jaw, which was removed by operation; the patient, however, a girl of fifteen years, dying on the tenth day from erysipelas. The cavity of the antrum was completely obliterated by the osseous matter, which exhibited all the characters of the natural texture. The general health was perfectly good, and no disease was discoverable in the surrounding parts. The hypertrophy had been in progress for eight years, and presented itself mainly in an oblong, hard, and painless enlargement of the ascending portion of the bone, projecting into the face, and extending into the nostril.

Such enlargements, it is obvious, are not malignant, and are, in general, easily distinguished from this class of affections by their slow development, their firm, unyielding consistence, their painless character, and the healthy condition of the adjacent parts.

7.—*Scirrhus* of the superior jaw must be exceedingly rare, as I have not, in a practice of nearly twenty-five years, met with a solitary case. Many instances of the kind are, I am aware, reported in our surgical and periodical literature; but it is perfectly clear to my mind that they were examples, not of scirrhus, but of encephaloid. Should this disease ever occur here, it would be likely to show itself in advanced life, as a hard, firm, solid tumor, slow in its progress, and characterized by sharp, lancinating pain. The growth would be likely to attain much less bulk than soft cancer; nor would it be so liable to fungate and bleed.

8.—*Melanosis* of the upper jaw is likewise very unfrequent, and no example of it has yet fallen under my observation. The same remark is applicable to colloid, which is, if possible, a still more rare disease than melanosis. A fatty tumor has been described as occurring in the upper jaw; and Gensoul gives an instance wherein

*Treatise on the Diseases of the Bones, p. 227; Phil: 1849.

the antrum was occupied by an erectile growth. The tumor, in this case, was remarkably soft, and so vascular that it appeared, after having been injected with quicksilver, to be composed chiefly of minute vessels.

9. *Polypoid* tumors of the antrum are occasionally seen, but much less frequently than is generally supposed. Indeed, they must be exceedingly rare. They are benign or malignant, generally the latter; are difficult to diagnosticate; and are always prone to return after extirpation. These tumors require no particular description; because they do not, in the great majority of instances, differ from the medullary formation, from which, in fact, it is usually impossible to distinguish them.

It would seem that *exsection* of the upper jaw was practised by Acoluthus as early as 1693. The operation was repeated, in the eighteenth century, by Planque, David, and Beaupreau, and, early in the present century, by Siebold, Deschamps and Klein.* In all these instances, however, it was limited to the removal of a portion of the bone. In Great Britain, extirpation of the upper jaw does not appear to have been practised until 1826, when it was executed by Mr. Lizars, of Edinburgh, who, I believe, is fully entitled to the merit of having first suggested the possibility and advantage of removing the *entire* bone, when the seat of disease.† Three years prior to this period our countryman, Dr. Alexander H. Stevens, then Professor of Surgery in the University of New York, excised nearly the whole of the upper jaw, along with portions of the malar, ethmoid, and sphenoid bones, for a fungous tumor of the antrum.‡ In May, 1824, Dr.

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*Velpeau's Operative Surgery, 2. p. 728.

†Liston's paper on Tumors of the Mouth, in London Medico-Chirur. Trans. 20, p. 178.

‡Cooper's Surgical Dictionary, Appendix by Dr. Reese.

David L. Rogers, of New York, performed a similar, but still more formidable operation, exsecting the superior maxillary bones, anterior to the first molar tooth on each side, together with the two inferior turbinated bones, a part of the nasal septum, the vomer, and a portion of the right antrum.* Since the period here adverted to, the operation in question has been repeatedly executed by American surgeons, among whom McClellan, Mott, Mussey, Mütter, Pancoast, and the two Warrens, deserve special mention. I am not aware that it has been performed by any one in Kentucky, except myself.

Previously to undertaking any operation for the removal of the upper jaw, whether for a part or the whole of it, a careful inquiry should be instituted into the history of every case, with a view of ascertaining its particular character, the condition of the adjacent parts, and, above all, the state of the general system. If the tumor be of the cephalomatous kind, large in size, rapid in its development, dipping into the surrounding structures, and attended with contamination of the neighboring absorbent glands, failure of the general health, and a sallow cadaverous countenance, all manual interference is out of the question. To operate, under such circumstances, would only hasten the patient's fate, and tend to bring surgery into discredit. On the other hand, however, there is no reason, in my opinion, for refraining from the use of the knife, even in this form of disease, provided the morbid growth is comparatively small and tardy in its progress, and the surrounding parts and constitution are perfectly sound. It is a well ascertained fact, as was previously stated, that encephaloid of the upper jaw rarely co exists with malignant disease in other organs; and, hence, if such a tumor be exsected at an early period of its development

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*Surgical Essays, p. 81, New York, 1848.

while its action is strictly local, success must occasionally crown our efforts, at least so far as to prolong the patient's life, if nothing more, beyond the period he could otherwise attain. The propriety of such a course is fully sanctioned by the experience of the best surgeons. In the case of Mrs. Green, the third on my list, although the disease for which the bone was removed was unquestionably of an encephaloid nature, and has repeatedly returned, five years have elapsed since the first operation, but for which she must have perished long ago. The great fault, in regard to the operation, is, that it is generally not resorted to sufficiently early in the disease, owing to the ignorance of the patient, and his unwillingness to submit to the knife as long as he can persuade himself that he may be relieved by ordinary means. The case being thus trifled with, the surgeon, when it falls into his hands, finds himself in the disagreeable dilemma of being obliged to decline all interference, or to attempt an operation, the result of which is almost sure to be unsatisfactory. He has to contend with science and self-interest, on the one hand, and sympathy for his patient, on the other; and his decision will be influenced by the predominance of the one or the other. Most men, if not wholly governed by selfish considerations, would be likely, if the case is not altogether desperate, to afford the sufferer a chance for his life. I do not wish to be understood, in making this remark, that I am an advocate for the indiscriminate use of the knife; by no means; on the contrary, as is well known to my friends and pupils, no one more constantly and loudly condemns a reckless and unprincipled resort to instruments. I merely wish to say, and I say it boldly, that I abhor timid surgery, and that cautious, calculating conduct in operators which induces them to pay more regard to their own reputation than to the welfare of their patient; operators who would not hesitate to consign the poor sufferer to his cruel fate, because they are afraid of public

opinion. I do not envy such men their feelings; I despise them as much, and as cordially, as I despise the merest knives-men. It should never be forgotten that a case, which may appear hopeless in the eyes of one surgeon, may not appear so in the eyes of another, equally enlightened; and that a case, which one surgeon has condemned as unsuitable for an operation, has sometimes been easily relieved by another equally expert. It cannot be denied that surgery has achieved some of its proudest triumphs in this manner.

In non-malignant tumors of the upper jaw, no fears need be entertained about a relapse; the only difficulty here is about the diagnosis. When this has been clearly established, and the patient is in a proper condition for an operation, the sooner it is performed the better.

The *manner* in which the operation is to be performed, must necessarily vary with the circumstances of each individual case. When the tumor is of considerable size, requiring sometime for its removal, the patient should always be placed recumbent, with a broad and rather thin pillow under the head and shoulders, and the face inclining to the opposite side. Very few persons, whatever may be their courage and fortitude, can bear the shock and fatigue of an operation of this kind while sitting up. The recumbent position is more particularly necessary when chloroform is administered. I find considerable objection expressed by certain gentlemen against the use of this article in operations upon the mouth; but without, it seems to me, any just reason; at any rate, I have resorted to this agent ever since its introduction into the materia medica in every severe case of amputation, both of the upper and lower jaw, that has come under my observation, and I have, thus far, had no cause to regret the practice. The only objection that can be urged against anæsthetics in these operations is, that the patient, being unconscious, is unable to clear his mouth of blood; but if proper care

be taken to compress the vessels as soon as they are divided, and the head be placed upon a low pillow, no danger can ensue from this source; I certainly have never witnessed any, and until I do I shall not hesitate to continue the practice, whatever others may say and do.

I have never found it necessary, in any of my operations upon the upper and lower jaw, to secure the carotid artery, as a means of preventing hemorrhage. Indeed, one cannot but be surprised that such a procedure should ever have been recommended, much less employed, by any sensible surgeon. My experience is that there are no organs in the body, of the same extent in their natural and diseased condition, the removal of which is attended with so little hemorrhage. I am not in the habit even of employing compression of the carotid artery in these operations; and, as to tying that vessel as a means of security against the loss of blood, I should as soon think of ligating the femoral artery for the same purpose. Nothing could be more absurd and unnecessary. The chief danger from bleeding is in the subcutaneous arteries, especially the facial and its branches, and expert assistants should always be at hand to seize and compress them as soon as they are divided. I rarely, if ever, stop to tie a vessel during any operation, however extensive, or complicated. My experience has taught me that there is, in general, no necessity for such a course, which is always attended with vexatious delay and annoyance. The deep seated arteries, involved in tumors of the upper jaw, seldom bleed much, if care be taken to keep beyond the limits of the diseased structures. If this precaution be neglected, the hemorrhage may be copious, and even exhausting. The oozing which takes place from the osseous surface, after exsection has been effected, generally speedily ceases of its own accord from the contact of the atmosphere; where this is not the case, a stop may usually be easily put to it by the application of compresses, wet with a saturated

solution of alum. The actual cautery could be necessary, in such a case only, when a portion of the tumor has been left behind; a circumstance which ought never to happen in the hands of any one, as it must necessarily lead to a speedy reproduction of the mischief.

The direction, extent, and number of the *incisions* through the soft parts must, of necessity, vary with the situation and extent of the tumor. In all these respects, much must be left, in each case, to the judgment and experience of the operator. When the morbid growth is comparatively limited, and seated upon the anterior, or antero-lateral aspect of the jaw, we shall generally be able to dispense with external incisions altogether, as our object may be readily enough accomplished simply by dissecting off the lip from its attachments, and holding it out of the way with the finger, or blunt hook. The surface of the tumor having been thus thoroughly denuded, the bone is to be attacked with the saw, and severed fairly beyond the line of disease. By this procedure, which is admirably adapted to the more simple forms of morbid growth, the operation is deprived of much of its severity, and followed by no deformity of the features, except what may result from the caving in of the integuments.

When the tumor involves the body of the bone, and is of considerable bulk, as, for example, as big as a fist, the plan which I usually adopt, and which has always answered my purpose most fully, is to make one long, curvilinear incision, extending across the most prominent part of the tumor, from the commissure of the lips towards the zygomatic process of the malar bone, terminating within a few lines, half an inch, or an inch of the external angle of the eye, according to the volume of the abnormal mass. In this manner are formed two flaps, the upper of which is convex, and the lower concave, which are then carefully dissected up by bold and rapid strokes of the knife, and held out of the way by trust-worthy assistants, who at the

same time take care to compress the bleeding vessels. The space which this procedure affords is, in general, quite sufficient for the easy removal of the entire tumor, however large, or extensive its connections. In my own cases, it has always answered my purpose most thoroughly. Should it, however, be inadequate, it can be easily increased to the requisite extent by a horizontal cut along the inferior border of the orbit, as far as the nose. In making the first of these incisions, the facial artery is necessarily divided, and in the second, the superior maxillary nerve, together with many of the branches of the portio dura of the seventh pair. In consequence of the injury thus sustained, the parts supplied by these nerves remain a long time paralyzed, though ultimately the face regains, in great degree, its accustomed power and expression.

Some surgeons recommend two incisions under the above circumstances; but such a procedure is not only unnecessary as it respects the facility of excision of the tumor, but exceedingly objectionable on account of the greater deformity to which it must lead.

When the tumor, or enlargement, occupies the anterior and upper portion of the jaw, the external incision may extend vertically upwards, by the side of the nose, from the free border of the lip to a level with the orbit of the eye. This will enable the operator to detach the ala of the nose, and to remove, if necessary, the ascending process of the jaw bone, the ungual bone, the inferior turbinated bone, and even the vomer.

When the antrum is mainly implicated in the disease, two incisions, representing the form of an inverted L, are necessary, the vertical limb corresponding with the ascending process of the maxillary bone, and the horizontal one with the inferior border of the orbit of the eye.

Whatever be the form and direction of the incisions, care should be taken that they are always sufficiently ex-

tensive to afford ready access to the diseased mass. Nothing can be more embarrassing, or display greater want of judgment in the operator, than a want of room in a case of this kind.

The necessary incisions having been made through the soft parts, and the flaps properly dissected up, the next step in the operation is to remove the tumor. As a preliminary measure, one or more teeth must be extracted, to make room for the play of the saw and other instruments. This part of the operation ought always to be performed as soon as the patient is fairly under the influence of chloroform, prior to the division of the soft structures. Performed after that, it is liable to occasion delay and annoyance.

The separation of the jaw is usually the work of a few minutes. The limits of the disease being generally well defined, care must be taken to keep on the outside of them, for the double purpose of avoiding hemorrhage, and removing the whole of the morbid structure. The best contrivance for executing this part of the operation is a saw, the blade of which is about three inches long, eight lines in width, and a little rounded off at the end, with sharp, wide-set teeth. It should be provided with a stout handle. The surgeon should be furnished with a series of instruments of this kind, as one is rarely sufficient. He should also have several chisels, a pair of pliers, a lenticular, and a stout scalpel, with a piece of steel at the extremity of the handle, which should be bevelled off, that it may be used as a scraper and a cutter, as may be deemed necessary. Mr. Liston has declaimed quite eloquently against the chisel and in favor of the pliers in this operation; probably, because the latter instrument was his own invention, and one of his well-known hobbies! I have used it on two occasions upon the upper jaw, but it failed so utterly of its purpose, and produced such a terrible noise and crash that I never think

of it without horror. The chisel may have its faults; it may jar and shake the head; but I should be loth to believe that it could possibly jar and bruise the parts as much as a pair of the best pliers, however carefully and gently managed.

When it is designed to remove the whole jaw, the saw should be carried, successively, through the alveolar process in front, and the horizontal plate behind, close to the middle line, as far back as the corresponding portion of the palate bone; the mucous membrane of the roof of the mouth having been previously divided with the scalpel, to prevent it from being torn and bruised, as it otherwise would be. Next the saw is to be applied to the malar bone, at or near its junction with the maxillary, and finally to the nasal process, which is generally divided on a level with the lower margin of the orbit of the eye. The orbitar plate of the jaw bone is commonly left intact, as it does not often participate in the morbid action. Should it do so, it should be cautionsly removed with the chisel and knife, lest the eye and its appendages sustain mischief. All that now remains to be done, is to sever the tumor at its junction with the pterygoid process and palate bone; and here, again, the chisel and knife will come in excellent play. The main tumor having been removed, the parts are carefully sponged, and any remnants of diseased substance cleared away with the lenticular and other suitable instruments. If a particle of the new deposit be suffered to remain, the operation will prove a failure, as it will be certain to be soon followed by a reproduction of the tumor.

The next step is to sponge away the blood, and to search for, and tie any vessels that may seem inclined to bleed. It is seldom, in any case, that more than two or three ligatures will be required. To arrest the oozing of blood from the deep portion of the wound, and give support to the cheek, I am in the habit constantly of stuffing

the osseous gap with patent lint, wet with a saturated solution of alum. The edges of the cutaneous wound are then approximated by twisted sutures and collodion strips; and a compress being applied upon the cheek, the parts are supported by a roller, passed round the head and under the chin in the form of a figure 8.

The *after treatment* must be conducted upon strictly antiphlogistic principles; the head and shoulders are maintained in an elevated position and as quiet as possible; light diet and cooling drinks are enjoined; the bowels are moved by aperients; and the secretions are corrected, if necessary, with calomel, or blue mass. The needles are removed at the end of the fourth day, when the edges of the incision will generally be found perfectly united. The great danger after this operation is erysipelas of the face and head, which commonly shows itself within the first forty-eight hours, and should be promptly met by the usual remedies. Several of my cases suffered severely from this cause. Few patients die from the shock of the operation. The subject of my sixth case died from pneumonia nearly three weeks after the removal of the whole of the upper jaw, together with the inferior turbinated and a portion of the malar and palate bones.

The patient soon becomes accustomed to his loss; and the function of deglutition, at first so difficult and annoying, is gradually performed with nearly its original facility. Even the faculty of mastication is regained much more rapidly than one, unacquainted with the compensating powers of nature, might be lead to suppose. The deformity of the face is often comparatively trifling; and the defect in the mouth may usually be remedied, in the more favorable cases, by artificial means. It is surprising how much, even in a short time, the cavern contracts and diminishes, and how all the surrounding and associated parts accommodate themselves to their new situation.

It would be interesting to give an account of the *re-*

sults of the different operations that have been performed for the removal of the upper jaw in malignant and other diseases; but for such an undertaking we have, unfortunately, no precise data, nor could such data be obtained without a most careful and elaborate analysis of all the reported cases; a task for which I have neither leisure nor inclination. When the malady is of the encephaloid character, it may be safely assumed that it will return, sooner or later, in almost every instance, no matter how thoroughly the abnormal structure may have been extirpated. In the non-malignant varieties of tumor, on the contrary, there is no reason to apprehend a relapse, any more than in the same class of affections in other parts of the body. Of eleven cases operated on by Lizars, Syme, Robert, Scott, Earle, Guthrie, and Hetling, only one was completely successful. Gensoul has removed the upper jaw four times, with one case of failure, and in that the affection was of a malignant character. Dieffenbach reports five instances, all successful; and the late Mr. Liston, seven, in only one of which the disease returned, and proved fatal. Rignoli has one successful case, and one failure.*

Looking at my own cases, I find that four have died; three from a recurrence of the disease, and one from the effects of pneumonia, nearly three weeks after the operation; that two have entirely recovered; and that one has suffered a relapse but is still living, five years after the extirpation of the morbid growth. In this case, that of Mrs. Green, the third on the list, I excised nearly the whole of the jaw in 1847, and since then several pieces have been removed, at different intervals, by Dr. Sloan, of New-Albany. Although the tumor, in this instance, was not inspected with the microscope, yet I am certain, from a

*British and Foreign Medical Review, 7. 250.

careful examination of its physical properties, that it was cephalomatous in its character; a circumstance which imparts more than ordinary interest to the case, inasmuch as it holds out great encouragement to the surgeon in an affection usually regarded as desperate, if not hopeless. In a case of brain-like tumor of the upper jaw, reported by Dieffenbach, the patient was subjected to five different operations before a final cure was effected.

CASES OF EXCISION OF THE UPPER JAW.

I shall close these remarks with a history of seven cases of excision of the upper jaw, which have been drawn up with great care, and the results of which have been satisfactorily ascertained in every instance.

Case 1.—The following case is remarkable chiefly on account of the cause of the tumor, which was quite small. Miss C., aged twenty-one, of Frankfort, Kentucky, came under my observation in June, 1843, for an enlargement of the left upper jaw-bone, which she first noticed about two years and a half previously; it occupied the alveolar process, was free from pain and discoloration, and produced no appreciable prominence on the corresponding side of the face until eighteen months ago. About six months after the tumor first attracted attention, the first bicuspid tooth became loose, and was extracted by a dentist; the operation was followed by considerable hemorrhage, but this soon ceased, and the parts healed in a short time. After this, the tumor increased a little more rapidly, and by degrees the second bicuspid became loose; but, as it did not ache, it was not removed.

At the time of Miss C.'s visit, the tumor was about the volume of a large hickory nut, and seemed to be formed at the expense mainly of the outer surface of the alveolar process; it was hard and inelastic, devoid of pain or soreness, and unaccompanied by any disease of the gum. The general health was, and, in fact, always had been, excellent.

As the tumor was making steady, though slow progress, and was productive of some deformity of the lip, as well as of great mental disquietude, I determined at once to remove it. As a preliminary measure, the canine tooth was extracted by my friend, Dr. Somerby. The bone was then sawed through, at the site of this tooth, as far as the inner table of the alveolar process, and to the same extent at the posterior limit of the tumor, which was then raised with a chisel and a strong knife, the lip having been previously detached from its upper extremity. As the internal lamella of the bone was perfectly sound, it was, of course, permitted to remain, especially when it was ascertained that the whole trouble had been occasioned by the presence of a tooth, apparently a cuspid, a little smaller than natural, but well formed, with the crown reversed, or directed upwards towards the antrum of Highmore. The parts soon healed after the operation, and the patient recovered with hardly any defect, except what resulted from the extraction of the tooth at the time of the operation. Could the real nature of the tumor have been determined before the operation, this tooth would, of course, have been saved.

Supernumerary teeth are not of unfrequent occurrence, especially in the upper-jaw; they are much more common among the permanent than the temporary set; and their shape generally resembles the cuspid or canine teeth. They are not, however, confined to the front of the jaw, though they are more frequent here than elsewhere. Inverted teeth are seldom met with. Dr. Somerby and Dr. Goddard, two eminent and experienced dentists of this city, inform me that they have never met with an example. Mr. Hunter* gives a drawing of the upper jaw of a child, in which the cuspid tooth was inverted, "so that its

*Natural History of the Teeth, Tab. 8, Fig. 9.

point was turned up against the jaw, and the growing mouth of its cavity towards the gum." Albinus relates a singular instance where a tooth was found on each side in the ascending process of the maxillary bone, between the nose and the orbit; it was long and very thick, and situated in such a manner that the crown looked upwards towards the eye. No satisfactory explanation has yet been furnished of the manner in which this vicious direction of the teeth is brought about. The subject is interesting in connection with that of osseous tumors of the superior maxillary bone.

Case 2.—Miss B., aged nineteen, of a florid complexion, and rather delicate health, from the vicinity of Shelbyville, Kentucky, observed, nine months ago, several weeks after a severe attack of bilious fever, a small tumor in the right upper jaw, which gradually increased in size, and at length formed a slight prominence under the cheek. At her visit to me in April, 1845, the parts exhibited the following appearances: The tumor, which was firm and solid, projected internally a little beyond the middle line, while at the side it extended from the anterior edge of the lateral incisor to the horizontal plate of the palate bone. The alveolar process was much expanded, and the middle grinder, having become loose, had been extracted by the family physician, under the belief that the disease was an abscess of the antrum of Highmore. The other teeth were sound, and firm in their sockets. The integuments of the cheek and the mucous membrane of the mouth were healthy. The speech and deglutition seemed to be little, if at all, affected. The tumor was the seat of slight, darting pains, which had latterly become more constant and severe, and its volume had nearly doubled itself during the last eight or nine weeks. The general health had not materially suffered.

Having properly prepared the system, I performed the operation of excision of the affected parts on the 19th of

April, 1845, in the presence of Drs. Johnston, Cochran, Somerby, Colescott, and T. L. Caldwell, the latter three of whom afforded me their kind assistance. The patient being a woman of unusual fortitude and determination, sat upon a chair, with her head reclining against the breast of one of the gentlemen just named. As a preliminary step, Dr. Somerby extracted the lateral incisor and last molar tooth. An incision was then made over the prominent portion of the swelling, beginning at the commissure of the mouth, and terminating near the outer angle of the eye, its direction being slightly curvilinear. The two flaps being next detached, and held out of the way, the jaw was divided, with a Heys' saw, by the side of the middle incisor, from whence the instrument was carried through the horizontal plate, close along the raphe, as far back as the palate bone. The tumor was then attacked with the chisel and mallet by cutting through the outer wall of the antrum, the cavity of which was occupied by a soft fungous mass, and was evidently the original seat of the whole mischief. This being thoroughly cleared out with the lenticular and handle of the scalpel, the operation was completed by sawing the jaw at its junction with the malar and palate bones. An artery, a branch of the one which supplies the vomer, was cut, and required torsion. No vessels were tied. The quantity of blood lost did not exceed four or five ounces. The woman bore the operation admirably.

The cavity left by the removal of the tumor was filled with patent lint, and the wound closed with four twisted sutures, aided by a compress and roller. As there was a good deal of pain, a grain of morphia was administered after the girl went to bed.

Everything went on well until the fourth day, when erysipelas, which had been for sometime epidemic in the city, made its appearance at the upper angle of the wound, and gradually extended over the whole face, ears, and

forehead. Under the influence of a few active mercurial cathartics and the application of the tincture of iodine, the disease was soon arrested, and comfort restored to the part and system. The sutures were removed on the fourth day, when the union was found to be complete.

As soon as suppuration was fully established in the bony cavity of the mouth, the lint was removed, and after the parts were well syringed with a solution of chloride of soda, it was replaced, to afford support to the cheek. These dressings were renewed daily for upwards of a week.

Miss B. left Louisville in a fortnight after the operation, almost as well as ever, and with hardly any perceptible scar, so perfect was the union of the wound. There was, of course, a considerable retrocession of the cheek, and great impediment in articulation; the bottom of the osseous cavity was covered with healthy granulations, and there was still some discharge of pus.

The tumor was carefully examined after removal, and exhibited all the characteristic features of encephaloid. Indeed, no morbid growth that I have ever inspected was better marked in this respect.

I saw this young lady four years after the operation, and am happy to say that she is still living, her residence being the neighborhood of Paducah. Her articulation is greatly improved, and the deformity of the face is so slight as to escape casual observation.

Case 3.—Mrs. Susan Green, aged thirty-eight, a married woman, the mother of nine children, from Crawford county, Indiana, visited Louisville in June, 1847, to consult me respecting a tumor of the left superior maxillary bone, which had of late become a source of much distress to her. Her statement was that the affection was first noticed in 1830, as a small, movable lump, situated opposite the second large grinder, between the median line and the alveolar process. The growth was slow and

gradual until about four years ago, when, after cauterizing it, for the third time, with nitrate of silver, it began to advance very rapidly, and has continued so until the present time. The tumor is now as big as an ordinary fist, and produces great distortion of the features; protruding the ball of the eye, pushing out the cheek, and closing the left nasal cavity. Internally it overlaps the right raphe of the roof of the mouth, while posteriorly it projects slightly beyond the maxillo-palatine suture, displacing the tongue, and thrusting it over towards the right side. Mastication and deglutition are both considerably impeded. The tumor is hard, firm, and inelastic. Last winter, the two bicuspid and two anterior molar teeth became loose, and were extracted, without causing any particular pain, or serious hemorrhage.

Until about six months ago, the tumor was free from suffering, except that it was the seat, pretty constantly, of a feeling of tension or tightness. Since then it has become affected with a dull aching pain, which radiates about in different directions, and is particularly severe in the left malar bone, and corresponding nostril. Mrs. Green is not aware that her jaw has ever received any injury, and she knows of no cause of the disease. Until a few months age, her health was always remarkably good; of late, she has lost flesh, and her countenance has assumed a rather sallow aspect.

Excision of the tumor was performed on the 15th of June, with the aid of my pupils, Dr. Summers, Dr. Bozeman, and Dr. Ector, and in the presence of a number of physicians and students. Dr. Goddard had the kindness to extract, as a preliminary measure, the canine and last molar teeth. The patient being placed upon a table, with her head resting upon a pillow, and being fully under the influence of ether, an incision was made over the prominent portion of the tumor, from the left angle of the mouth to within a short distance of the outer canthus of

the eye, in a slightly curvilinear direction, the convexity of the curve looking downwards. The flaps being dissected off, the saw was applied to the anterior and lateral part of the jaw, from whence it was carried obliquely backwards through the raphe of the horizontal lamella of the maxillary bones to a little beyond their junction with the palate bones. Continuing the operation, I next divided, with the saw and pliers, the root of the ascending process of the bone, nearly on a level with the floor of the orbit, and finally the malar process and a part of the horizontal plate of the palate bone. Having thus severed its principal and most important connections, the tumor was wrenched from its bed with the hand and chisel, and the bottom and sides of the chasm immediately cleared of every thing betraying the slightest appearance of disease. A good deal of oozing of blood took place at this stage of the procedure, but this soon ceased spontaneously, and did not afterwards recur.

The osseous cavity being stuffed with lint, wet with a saturated solution of alum, the edges of the wound were brought together by five twisted sutures, and the woman, somewhat exhausted by the operation, removed to her bed. Not more than five or six ounces of blood were lost during the operation, which occupied but a few minutes. No arteries, except a small muscular branch, required the ligature. Some vomiting occurred at the close of the operation, from the effects, apparently, of the ether. As soon as this had passed off, a grain of morphia was given, to allay pain, which was now considerable, and mustard was applied to the chest and extremities.

It is not necessary to present a diary of the symptoms and treatment of this case, which progressed most favorably, with the exception of a slight attack of erysipelas of the face, from the time of the operation till the patient left Kentucky, nearly three weeks after. The sutures were withdrawn at the end of the fourth day, when the wound

was found to have united, in its whole extent, by the first intention. The lint was removed from the mouth as soon as the suppurative process had fairly begun, and was re-inserted daily for a week, when its employment appearing no longer necessary, it was dispensed with. As a detergent, free use was made of the chloride of lime in solution. When the patient went home, the bottom of the chasm was covered with fine healthy granulations, and nearly all the discharge of pus had ceased. The face was but slightly disfigured. The voice was, of course, materially changed, and there were many words which she was unable to pronounce with distinctness.

Having learned that Mrs. Green had suffered a relapse, and that she had become a patient of Dr. Sloan, of New Albany, I addressed a note to that gentleman, who kindly furnished me with the following facts: "Mrs. Green called upon me on the 4th of March, 1849, with a considerable enlargement of the median portion of the alveolar process of the left upper jaw, which I removed the next day, by making a vertical incision from below the inner angle of the eye to the border of the left lip, detaching the ala and septum of the nose, and then cutting the bone obliquely across from between the incisors on the right side, through the nasal cavity, to the opening made in your operation. The piece thus removed contained, I think, the roots of the canine and two left incisor teeth, with the central incisor of the right side.* About eight months after, she applied to me for a small growth on the posterior boundary of the cavity, which I removed with its bony attachments.

I saw Mrs. Green again in May, 1852. Her disease had

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*Dr. Sloan is, I think, in error in saying that the piece included the canine tooth, as I have a distinct recollection that I sawed the bone through the socket of that tooth, in my operation.

again returned, involving the posterior wall of the antrum, the palatine arch, and the pterygoid process, and extending towards the coronoid process of the lower jaw. I expect she will visit me next autumn."

Case 4.—William Collinridge, aged sixty-six, a brass-founder by occupation, and a married man, of temperate habits, and nervo-sanguineous temperament, was recommended to me by his professional adviser, Dr. Hagan, of this city, in the latter part of July, 1847, on account of a tumor in the right upper-jaw. He had always been healthy, until about four months ago, when he became feeble and nervous, and soon after discovered a slight swelling of the gum, attended with aching of the large grinders. His suffering increasing, he consulted a physician, who advised the extraction of the affected teeth, which was accordingly done, but with only temporary relief. The operation was attended with considerable hemorrhage, and was soon followed by an increase of the swelling, and a recurrence of the pain, in all its previous violence.

When Collinridge first consulted me, the tumor was so large as to form quite a prominence under the cheek, and to encroach a good deal upon the buccal cavity; his speech was thick and indistinct, and he was unable to masticate solid food. The enlargement extended from the lateral incisor to the posterior extremity of the bone, and had a softish and elastic feel, not unlike an encephaloid tumor. The mucous investment was remarkably florid and congested, but sound in texture, and the integuments of the face were perfectly healthy. The pains were excessive, and almost without intermission; darting about in different directions, and usually most severe at night and during damp and cloudy states of the weather. All the teeth on the right side, excepting the two incisors, had either dropped out or been extracted.

On the 9th of August, 1847, assisted by Dr. Hagan and

several of my private pupils, I excised the diseased mass, the patient having previously inhaled ether. The incision was made, as in the cases already described, from the commissure of the lips in a slightly curved direction, to within a short distance of the external angle of the eye. The flaps being dissected up, and held out of the way, the saw was applied in front, close by the central incisor, and thence carried back to the palate bone. The bone was then cut at its junction with the malar bone, at the ascending process, and along the orbitar plate, when the whole mass was forcd out of its bed. A considerable flow of blood took place, at this stage of the operation, which had to be stopped for a few minutes, until the patient recovered from the effects of the resulting exhaustion. A large sponge was immediately pressed upon the bleeding surface; and as soon as I was able to proceed, the blood was wiped away, and the osseous cavity cleared of its morbid contents with the knife, chisel, and lenticular. The hole was then stuffed with lint, wet with a saturated solution of alum, and the edges of the wound were approximated by a compress and bandage. Only two small arterial branches required the ligature.

Nothing of any special interest occurred after the operation, which was not followed by a single bad symptom. The wound united by the first intention, and the sutures were removed at the end of the fifth day. Gradually granulations sprung up at the bottom of the gap, greatly diminishing its depth and extent, and thus aiding in the recovery of the power of speech and deglutition. The general health rapidly improved, and the patient was perfectly free from the local suffering which had so much harrassed and distressed him before the operation. Things went on in this manner for about six weeks, when, all at once, a small tubercle made its appearance upon the skin, at the former site of the tumor, and soon acquired the size of a twenty-five cent piece; it was hard and slightly

elastic to the touch, projected some distance above the surrounding surface, was of a dark livid hue, and the seat of a constant sharp, shooting pain. No attempt was made to remove this, for it was but too evident that it was of a malignant character, and that any further operation would be worse than useless, the more especially as the general health now began to give way, and the granulations at the bottom of the wound presented a pale, flabby, and unnatural aspect. Soon after this, small tubercles, of a kind similar to that just mentioned, showed themselves upon the temple, rapidly increasing in size, and greatly aggravating the local distress. The pains were excessive, shooting and darting about in every direction, through the face and head, and requiring the constant use of morphia for their subjugation. The tumor on the cheek acquired a large bulk, and at length ulcerated, discharging a large quantity of sanguinolent fluid, and occasionally pure blood. Excessive emaciation supervened, followed by hectic irritation, and colliquative diarrhœa, under which the patient sunk in October, 1848, fourteen months after the operation. No examination was made.

The morbid mass, in this case, possessed all the external features of encephaloid. The bone was deprived of its normal texture, and the soft portion of the tumor was of the color and consistence of the adult brain.

Case 5.—The subject of this case was Sylvia, a negress, married, thirty-five years of age, and a servant of Mr. Stephen Brown, of Springfield, Kentucky. She came under my care in 1849.

The disease first appeared about three years ago, as a boil, on the inside of the gum, and has gradually but steadily attained its present dimensions. During the last few months, however, its growth has been much more rapid than before, and this, so far as can be ascertained, without any assignable cause. The tumor, at this time, embraces the jaw, from the right canine tooth to the ma-

lar articulation, filling up the zygomatic fossa, and hideously distorting the features. In front, it does not quite reach the middle line, but posteriorly it projects beyond the raphe, and extends as far back as the soft palate, which, together with the side of the pharynx, is red and tumid, causing much difficulty of articulation and deglutition. The largest portion of the tumor, however, is external to the jaw, within the lips and cheek, reaching from the zygomatic process of the temporal bone to the angle of the inferior maxillary bone behind, and to the anterior portion of the mouth in front. In its shape it is irregularly globular, its longest diameter, the antero-posterior, being about three inches and a half. Of a pearly pink appearance on the inside, it is of a deep red, almost purple color, externally beneath the cheek, and is traversed by numerous vessels, filled with dark blood. In its consistence it is hard and inelastic, except along the groove formed by the pressure of the teeth of the lower-jaw, where it is somewhat soft, and ulcerated. It has been more or less painful ever since its commencement, especially at the back part. Much distress has also been experienced, of late, in the forehead, temples, and nape of the neck.

During the progress of the tumor, the back molar tooth became loose, and was extracted, showing a small abscess filled with purulent matter. The operation afforded only temporary and partial relief, and was attended with a good deal of hemorrhage. Subsequently, the other grinders shared the same fate. A neighboring physician, supposing that the main disease was in the antrum, punctured it with a lancet, but nothing but blood issued from the wound.

About three months before her arrival in Louisville, a hard, circumscribed swelling, as I am informed by Dr. Hughes, of Springfield, appeared on the front of the neck, just above the sternum, slightly compressing the trachea, and impeding respiration. After sometime, fluc-

tuation was perceived, when it was opened, and soon after it gradually went away, except a little remnant, which is still discharging a thin, sanious fluid.

Although my patient's health was a good deal impaired, and the tumor was of an unpromising character, I considered it my duty to operate, being satisfied that, bad as the chances were for recovery, the case was not wholly hopeless. Accordingly, on the 12th of April, assisted by Drs. Colescott, T. G. Richardson, Henry, and Thomson, the woman was placed upon the table, and put under the influence of chloroform, the external lateral incisor having been previously extracted by Dr. Goddard. The external incision, curvilinear in its direction, extended from the commissure of the lips to within a few lines of the outer angle of the eye, over the most prominent portion of the morbid mass, the surface of which was fully exposed by the reflection of the two flaps of integuments. The bone being divided in front and internally, with a pair of sharp pliers, and the anterior wall of the antrum laid open with a stout knife, the tumor was turned out of its bed, followed instantly by a copious gush of blood, and the fainting of the patient. Pulling the pillow away from her head, and thrusting a sponge into the bottom of the chasm, I scraped out, with all possible haste, the remnants of soft structure, and then sawed the malar and palate bones; finishing the operation by dissecting out a process of the tumor which had dipped in between the arch of the palate and the pterygoid muscles, between the upper and lower jaw.

Two small arteries being secured, the blood was sponged away, and the osseous cavity stuffed with lint, wet with a saturated solution of alum. The edges of the wound were approximated by four twisted sutures and collodion plasters, and the patient, much revived by means of hartshorn and brandy, put to bed. A grain of morphia was administered soon after the operation, and until the following

morning the woman was most assidiously watched by my assistants, Drs. Thomson and Henry, whose kindness I had frequently witnessed on similar occasions. Complete reaction had taken place by 4 o'clock in the afternoon, four hours after the operation, and the night was passed in a comparatively tranquil and comfortable manner, there being but little pain and difficulty of deglutition. The pulse was eighty and soft; there was little thirst, and the woman had taken some gruel. The face and eyelids were slightly swollen.

On the 14th, two days after the operation, the patient had several free motions from the effect of an ounce of oil taken the previous evening; she had slept some, and there was no fever or thirst. The swelling of the face and eyelids was the same as on the 13th. On the 15th, the patient expressed herself quite comfortable, and the parts looked well. On the 16th, I withdrew the needles, and removed the lint from the mouth. The next day, the fifth from the operation, the collodion plasters came off, thus exposing the wound, which was found completely united by the first intention.

After this period no diary was kept; the case went on well; and, in less than three weeks, the woman returned to her master, rapidly improving in strength and flesh. Sometime afterwards, as I learn from Dr. Montgomery, of Springfield, the disease reappeared in the roof of the mouth, in the form of a small excrescence, which rapidly increased until it attained the volume of a pullet's egg. It was attended with severe pain in the head, and great swelling of the nose, which looked as if it were distended with a large polypus. The corresponding eye participated in the disease, and finally burst. There was great difficulty of breathing; and, about four weeks before the woman died, the disease showed itself in the forehead and opposite eye. Death took place on the 15th of December, 1850, twenty months after the operation.

Case 6.—Benjamin Hudson, of Alabama, aged fifty-two, a farmer, married, and the father of eleven children, of a sanguineo-bilious temperament, dark complexion, and temperate habits, was always healthy until the latter part of November, 1850, when he perceived, without any assignable cause, a small circumscribed swelling in the right cheek, a short distance below the outer corner of the eye, and just within the malar bone. The face was sore and tender to the touch, but the skin was free from discoloration. About three weeks after he observed this swelling, he noticed, for the first time, a little tumor, about the size of the end of a finger, hard and firm in its consistence, situated under the cheek in contact with the gum. In fact, it looked and felt like a gum-boil. It bled upon the slightest touch; indeed, sometimes spontaneously; now slightly, and then quite profusely. Increasing gradually but steadily in volume, it at length encroached upon, and loosened the anterior and posterior molar teeth, so as to render it necessary to extract them in February, 1851; the middle one having been removed about eighteen years previously, along with two small fragments of the jaw-bone. By the middle of April, the tumor had attained the volume of a goose's egg, and was making most rapid progress. The pain which attended it was of a sharp, lancinating character, and was so violent and incessant as to deprive the patient almost entirely of appetite and sleep. He had also by this time lost a great deal of flesh and strength.

The patient reached Louisville on the 3d of May, 1851, in the following condition. The right cheek was occupied by an enormous tumor, of an elongated ovoidal shape, being longer in the antero-posterior than in the vertical diameter; it was very tender on pressure, and the seat of a severe and constant pain, sharp and darting in its character. The integuments were of a purple color, and the corresponding portion of the upper lip was very

much swollen and distorted. The tumor extended up to the eye, backwards over the zygomatic process, forwards to within a few lines of the nose, and downwards towards the base of the lower-jaw. Viewed within the month, it was found to reach inwards to the middle line, forwards to the lateral incisor, backwards to the soft palate, and downwards into the floor of the buccal cavity. It was of a pale reddish complexion, firm and elastic, tender on pressure, and irregular in its shape. Towards its anterior and lateral aspects, where it lay in contact with the cheek, was an ulcer, about the size of an American eagle, the surface of which was of a brownish color, rough, and fungous. It was free from pain and hemorrhage, and the patient was unconscious of its existence. The tumor seriously impeded articulation and deglutition, but the respiration was natural. The patient had no appetite, and he had not slept well since he left home; he was thin, and his countenance, although not sallow, had a haggard, care-worn expression.

Having purged and dieted my patient for nearly a week, I operated upon him in the presence of Drs. Hardin, Raphael, Thomson, Richardson, and other medical gentlemen. Chloroform was given him to the requisite extent; and, as a preliminary step, Dr. Goddard extracted the central incisor. By means of a curvilinear incision, extending from the commissure of the lips to the zygomatic process, an inch and a half from the outer canthus of the eye, two flaps were marked out, which were then dissected up, and carefully held out of the way. The jaw being sawed through in front at the middle line, the instrument was then carried through the horizontal plate of the *left* jaw bone, close along the raphe, and back through the entire length of the corresponding portion of the palate bone. The next step of the operation consisted in dividing the maxillary process of the malar bone, the periosteum of which appeared to be involved in the

disease. The nasal process of the jaw bone being now sawed, just below the lacrymal sac, the morbid mass was attacked with the chisel and mallet, and severed along with the orbitar plate, and the horizontal portion of the palate bone. The inferior turbinated bone, and a portion of the nasal septum were also detached. Finally, the operation, one of the most horrid I have ever executed, was completed by freeing the surface of the osseous cavity, with its various chasms and ramifications, of all diseased matter, with the knife, scoop, and lenticular.

The patient towards the close of the operation became faint, and was obliged to use cordials and other restoratives. The loss of blood did not exceed eight or ten ounces; only a few little arteries required to be secured. The bony cavity was stuffed with lint, wet with alum water, and the parts were approximated with six twisted sutures and collodion strips, supported by a compress and bandage.

As soon as the effects of the chloroform had passed off, the patient took a grain of morphia. In the evening I learned that he had been quite comfortable nearly all the afternoon; he had slept some, and had little or no thirst, nor much local uneasiness. The following night was passed in tolerable comfort; next day he had some fever; and he complained of a sense of tension in the deep portion of the wound. On the third day, slight erysipelas came on in the right side of the face, and rapidly increasing in extent, soon spread over the forehead and temple. Recovering from this disease, in a short time, every thing went on well for a fortnight, when the patient was seized with pneumonia, which gradually progressed from bad to worse, despite the means employed for arresting it, until the 28th of May, when it proved fatal. It is proper to observe that the wound healed nearly in its entire extent by the first intention.

An examination after death revealed high inflammation

of the lungs and pleura on both sides, with considerable effusion into the thoracic cavity. No carcinomatous disease was found any where, and the parts concerned in the operation exhibited a perfectly healthy aspect.

Case 7.—In the following case, which came under my observation in May, 1851, only a portion of the upper jaw was removed, the disease necessitating the operation having apparently commenced in the maxillary sinus.

J. H. P., of Henderson county, Kentucky, aged fifty-eight, a tall, slender, hard-working farmer, had his attention arrested in the early part of February, 1851, by a watering of the left eye, followed, soon after, by a severe, dull aching pain, confined, at first, entirely to that organ, but subsequently diffused over the upper part of the face, the forehead, and temple. He had always enjoyed good health up to this time, and was not aware that he had received any local injury. About the middle of April, he first noticed a swelling in the lower part of the orbit, and also a free discharge of muco-purulent matter from the left nostril.

Mr. P. arrived in Louisville, accompanied by his physician, Dr. Howard, of Owensboro, on the 24th of May. The upper portion of the left side of the face was, at this period, affected with a tumor, which encroached so much upon the corresponding eye as to protrude it partially from its socket, and which was the seat of a sharp, lancinating pain, darting about in different directions, and so constant and intense as to deprive him, in a great measure, of appetite and sleep. The eye had a watery, inflamed appearance. The integuments of the face were in a sound state. Upon inspecting the left nostril, I found it occupied by a large swelling, evidently a portion of the tumor already described, and discharging a large quantity of thin bloody fluid, of the most fœtid character. The general health was somewhat impaired, but the patient was in good spirits, and anxious for an operation.

Although I must confess that I did not like some of the symptoms of this case, and so expressed myself to Dr. Howard, yet such was the desire of the patient to be relieved, that I was at length prevailed upon to undertake an operation. Accordingly, having dieted him for a few days, and given him some purgative medicines, I proceeded to my task on Tuesday, the 27th of May, aided by my friends, Dr. T. G. Richardson, Dr. Thomson, Dr. Hardin, and Dr. Raphael. Chloroform having been administered, two incisions were made at right angles with each other, in the form of an inverted L, the limb of one extending along the side of the nose as low down as the lip, and that of the other along the margin of the orbit of the eye. The flap was then dissected up, and the wall of the antrum attacked with the saw, chisel, and lenticular, until the whole of the diseased structures, osseous and soft, was turned out. A process of the tumor dipped back deep into the orbit, and was removed with much difficulty, owing to the importance of the surrounding parts, and the manner in which they were obscured by the bleeding. The whole of the upper jaw, excepting the alveolar and horizontal portions, was cut away along with a part of the malar bone. A large artery, a branch of the internal maxillary, bled profusely at the bottom of the osseous cavity, and was finally, after much ado, secured with a ligature. In consequence of the great oozing of blood, it was necessary, before replacing the integuments, to stuff the cavity with lint, wet with a strong solution of alum. The edges of the flap were approximated with three interrupted sutures and collodion strips, supported by a compress and bandage.

The patient was put in bed, his head reclining upon a high pillow, and immediately took a grain of morphia. A short time after he lay down, blood was seen escaping from the nostril; but the quantity was small, and the discharge soon stopped of its own accord, and did not sub-

sequently recur. At the end of the fifth day, free suppuration being established, the lint was carefully withdrawn through the nose, and through a small opening in the upper limb of the wound, the greater portion of which had united by the first intention. The cavity was afterwards syringed daily with a weak solution of chloride of soda, under the use of which, together with mild tonics and a nourishing diet, the parts rapidly healed, and the general health greatly improved. The patient left Louisville on the 8th of June, with directions to pay the utmost attention to himself in every particular. Within a few days after he reached home, he was seized with a violent attack of dysentery, which was then epidemic in that region of country, and under which he sunk, completely exhausted, on the 2nd of July. Dr. Howard, of Owensboro, from whom I have learned these particulars, in a letter dated August 17, 1852, informs me that a tumor, about the volume of a medium-sized marble, and the seat of the most excruciating pain, appeared, about a week before he died, in the left orbit, just above the inner canthus.

I was not aware, until I received the above intelligence, that Mr. P. had died so soon after he left Louisville, and from a cause so entirely foreign to that of the disease for which I operated upon him. How long he might have lived, had he not been thus assailed, after the affection of the orbit showed itself, is a matter which must be left wholly to conjecture. Deducting this case from those which are stated, under the head of *results*, as having proved fatal, it will be perceived that only two died from the *immediate* effects of a relapse of the malady for which recourse was had to the knife.

It is proper to add that the tumor, in this case, exhibited all the physical and microscopical characters of encephaloid, as I was assured by my friend, Dr. T. G. Richardson, who carefully examined it within a few hours after its removal.

I had forgotten, until after the greater portion of this paper was printed, that Dr. Stevens, to whom, in common with some of my brethren, I have ascribed the credit of being the first to excise the upper jaw in this country, had been anticipated in this undertaking by Dr. Jameson, of Baltimore. In looking, accidentally, over the fourth volume of the American Medical Recorder, I find that this distinguished surgeon performed the operation in question in November, 1820, nearly three years prior to that of Dr. Stevens. The primitive carotid was tied as a precautionary measure, and the patient remained well when the case was reported, nearly three months after the operation. The account of the case is accompanied by a graphic engraving, showing the size and situation of the tumor, as well as the great deformity to which it gave rise.

Louisville, Aug. 24, 1852.

REVIEW.

ART. II.—*A Treatise on the Practice of Medicine.* By GEORGE B. WOOD, M. D., Professor of the Theory and Practice of Medicine in the University of Pennsylvania; President of the College of Physicians of Philadelphia, &c. &c. Third Edition. 2 vols. Philadelphia: Lippincott, Grambo & Co. 1852.

Practical works on medicine are ever likely to be more popular with practitioners of the healing art, than those of a theoretical character, and in the ratio that medical books, purporting to be of a practical character, approach what they assume to be, in that proportion are they likely to win a present, and to maintain an enduring popularity. Indeed, much is likely to be forgiven in a practical treatise, if it approaches to a redemption of its promises. When a medical reader finds himself under the guidance of a cool, calm, dispassionate observer of disease, to whom extensive opportunities have been given for grappling with disease; when these have been faithfully used; when such an observer recognises the noble mission given to him, and makes an honest record of what he has seen, we repeat, when a medical reader finds himself in the hands of such an instructor, his confidence is won, and his mind jumps at the opportunity for instruction. Authors of practical works in medicine often forget their high vocation, and instead of making their books the medium of imparting useful knowledge, they crowd it with incidents of personal achievements with means, that are rarely as

successful in the hands of others. Another class of these authors are the merest hobby-riders. They assume that all the world was blind up to the time of their advent, and they ride their hobby greatly to their own satisfaction.

There are few responsibilities in author-craft that equal those assumed by the medical writer, especially if his position be such as to lend weight to his views He is capable of inflicting no ordinary amount of evil upon the human family. Those who are familiar with the history of Galen, Celsus, Boerhaave, and Rush, are prepared to estimate the force of our remark. But when a medical writer makes truth, and truth alone the goal of his wishes, and uses his position to give force and efficiency to that great object, he deserves, and should receive the confidence and esteem of his professional brethren. Such an author we find in the writer of the work before us. He has long enjoyed a distinguished rank in his profession, and has never lost sight of what was due from that rank. He has constantly labored to advance the interests of his profession by every laudable method, and his great and useful labors have given him the respect, esteem and confidence of his brethren. They have stamped the seal of their approbation upon the labors of Professor Wood, in the field embraced in this work. In 1847, the first edition appeared, and in two years after, a new one was demanded, because the first was exhausted. In 1849, another edition was published, and though twice the size of the first, it is already exhausted, and a third is now needed. These facts are scarcely more complimentary to the labors of Professor Wood, than to the good sense, true taste, and proper appreciation of medical readers. The work richly deserves just such triumphs as it has won, and we hope it will long continue to enjoy the regard of the medical profession. Its popularity shows the discriminating studies of medical practitioners, and that the author who aims at a high mark may rest assured he wins everything by hitting it.

We have indicated some of the foundations on which books of this character should rest, and Professor Wood shows that these foundations belong to the structure he has erected. He says: "Having been engaged, for nearly thirty years, in public and private practice, and, during that time, devoted an almost exclusive attention to the study of diseases and their remedies, he has accumulated facts, and formed opinions, which have been long soliciting expression, with an urgency to which he has at length yielded, though unfeignedly distrustful of their sufficient value."

These are most excellent reasons for such a work as this of Professor Wood's, and a large portion of it will make the reader thankful that any reasons whatever, induced the publication. It is not to be expected that any work on medicine can be suited to every meridian. The influence of occupation, of habits of life, of methods of sustenance, of climate, and of race, is too well known to require much in the way of illustration. Let us take a simple example. The profession is largely indebted to Marshall Hall for his valuable work on blood-letting. That work contains a large amount of honest observation, most honestly recorded, but the Western or Southern practitioner, in this country, who should undertake to make it his guide, would condemn many a patient to weeks of unnecessary suffering, and commit more grave offences. Marshall Hall made his observations in the haunts of the crowded, ill-fed population of Manchester, where the sins of miserable ventilation alone would be sufficient to deter a medical man from the use of the lancet. It is quite as essential to know the circumstances that gave existence to a medical maxim; to understand the soil in which it grew, and to give everything its weight and proper bearing, as to know what the maxim is. When these things are forgotten, a high and honest authority may become a minister of evil, and Professor Wood is one of the last

men to ask any one to follow his suggestions thoughtlessly. An unthinking and undiscriminating physician is not, in the least degree, benefitted by reading unless he learns to think and to discriminate. We know many, who, in apparent despair of improvement in the latter elements, pursue undeviatingly the course of not reading.

It is not our purpose to enter into a critical analysis of the work of Professor Wood. Upon several important points, of great value to Southern and Western practitioners, Professor Wood is clear, cogent and, in the main, correct. His chapter on malaria is full of instruction, and richly deserves the profound study of every practitioner who is not already familiar with the laws of that Protean agent, and the diversified diathesis it assumes. Fatal mistakes are often made by those who neglect the study of these important subjects.

We find much to commend in the chapters on intermittent and remittent fevers. The plan commended by Professor Wood for the management of the former is not in our judgment the best. We have tried the use of small doses of quinine, frequently repeated, and of large and decided doses given at once, and not repeated, and we greatly prefer the latter. Nor is it essential to precede, in all cases, the use of the quinine by purgative remedies. We have had too many opportunities for comparing these various plans of treating intermittent disease, to be easily deceived about anything in relation to them. As a general, nay almost an universal rule, it is perfectly safe, and judicious to give decided doses of quinine immediately after the sweating stage of a paroxysm is well established, and the patient need have no fear of any evil consequence. If there is reason to apprehend a short interval between the close of the phenomena of one paroxysm, and the commencement of the next, there is no necessity for waiting for the sweating stage. In such cases it is *best* to begin the use of the quinine in the hot stage rather than

risk another paroxysm through want of enough interval.

The chapter upon remittent fever has our unqualified approbation. A physician who thinks for himself, and uses a proper degree of discrimination may, under the suggestions of this excellent chapter, grapple with remittent fever with great assurance of success.

The remarks of Professor Wood on typhoid fever are sound, judicious, and generally very correct, and we take great pleasure in commending them to medical readers.

These are the leading chapters that are likely to be attractive to Western readers, and they may resort to them with the certainty of finding good advice that will be serviceable to their patients.

Among the modifications of the third edition are revisions on the subject of epidemic cholera, and we regret that the Professor has permitted loose statements in regard to the philosophy of that disease, to creep into his work. An author capable of exercising the influence that Professor Wood may fairly claim, one, who like him is distinguished for fairness and truthfulness cannot be too careful in the statement of what purports to be facts. The uninformed, the unwary and unthinking seize upon statements made by such an authority as Professor Wood and soon learn to swear by them. A little care, some investigation, and a determination to be rigidly exact in all things, will, generally, save an author from grievous blunders. We should like to see the authority on which Professor Wood bases a portion of the following statement: "In general, its" (the cholera) "progress is arrested during the winter, to be resumed in the spring; though this is not invariably the case; for at Moscow it *prevailed* through most of the winter of 1830–'31; *the cold weather did not prevent its spreading along the Mississippi in the winter of* 1848–'49." We have marked in italics the objectionable features of this statement, and we are at a loss to know on what it rests. Cholera did not *prevail* in Mos-

cow in the winter of 1830–'31, and cold weather did prevent "its spreading along the Mississippi in the winter of 1848–'49." When cholera "prevailed" in December, 1848, in New Orleans, Dr. Fenner says, "the weather was *oppressively* warm," the thermometer being at 84°. Between the 12th of December, 1848, and the 20th of January, 1849, there were nearly fourteen hundred deaths in New Orleans, from cholera. Before the ravages commenced the weather became oppressively hot, and while it continued of that character, the pestilence was dreadful in its ravages. The scourge was arrested by white frosts early in January, and steadily declined under the influence of cold weather. And cholera did not spread anywhere, in 1848–'49, on the banks of the Mississippi, where there was cold weather. We had many cases of cholera here in Louisville, that winter, but *every* case was brought from New Orleans. There was not an attack of cholera, nor of anything approaching it, in Louisville, in the winter of 1848–'49, among citizens who were not in New Orleans during the hot weather of December. Nor was there any more cholera anywhere on the banks of the Mississippi, during "cold weather," in 1848–'49, than there was in the year ONE. Cholera is a hot weather disease, and never originates in any other kind of weather. It may be, but that is very rare, *developed* in cold weather, but that can only occur immediately after warm weather. If, in any climate, cold weather sets in early in autumn and continues up to December, cholera will not begin in December. We defy the production of any such occurrence.

If the Professor will take the pains to examine Fenner's Southern Medical Reports, and B iel's paper in the New York Medical Journal, on the prevalence of cholera in New Orleans in the winter of 1848–'49, and its prevalence in New York in 1849, he will find the following impressive and conclusive facts on the subject:

In New Orleans, in seventeen days from the com-

mencement of cholera, the deaths amounted to forty-five. As the weather increased in warmth, the disease spread and raged in both cities. From the 5th of December to the 16th of December the temperature gradually increased, and cholera increased slowly; from the 16th to the 22d of December the weather was oppressively warm in New Orleans, and cholera increased rapidly in its ravages, and spread itself over the city, and on the 28th reached its zenith. In the city of New York the temperature increased gradually, in May, and cholera increased gradually and slowly. Dr. Buel says: "during the first seven days of June the weather became warm, and the disease spread rapidly in all directions. Within this period seventy cases were received into the hospital, being double the whole number admitted during the whole previous time the hospital had been open. At the end of the first week in June, or about twenty-eight days from the appearance of the first case, the pestilence had become pretty extensively and generally diffused over the city. At the end of the third week in July, or about seventy-two days from its first appearance, it had arrived at its "culminating point." In New Orleans the increase of the temperature, after it began to rise, was rapid, and intense, and a corresponding state of things existed in the march of cholera. The disease in New Orleans was checked by a sudden check of the summer temperature. In New York the temperature and disease increased gradually and uniformly. Its decline depending upon the diminution of one of the elements essential to the production of the cause of cholera—moisture, was gradual and uniform, but more rapid than its increase. In New Orleans the element removed was solar heat, and as the removal was sudden, the effect was as sudden as could be expected where many had imbibed the cause without having succumbed to it, up to the time the farther production of the cause was stopped. In New York, the element re-

moved was moisture, and that was removed more slowly than the solar heat was in New Orleans, consequently the cause ceased its operations more gradually. In the production of the cause of cholera, too much moisture plays the same part that too small an amount does.

We earnestly hope that in a future edition of this valuable work, Professor Wood will show that he has thoroughly investigated the important point we have criticised. The statement on which we have animadverted is, one of too much moment to pass unnoticed. By what possible means is the medical profession ever to arrive at the etiology of cholera, if the phenomena are continually misrepresented, or perverted by loose and unguarded statements? If the premises are false, the deductions from them must be akin to them. However much we may now despair of truthful conclusions respecting the associated circumstances of cholera, we may hope for a better future, provided that hope has a proper basis. But if the future is to improve, the present must reform. Facts must be gathered, sifted and stated accurately. Hundreds of writers might be inaccurate with less injury to the cause of truth, than one such author as Professor Wood. He is not a mere theorist; he is no hobby rider, but is recognized as a man of truth; as a philosopher beyond the suspicion of misstatements. His medical works, too, are certain to live, and we should rejoice to see them passed down to posterity without a flaw to mar their merits, without an error to injure medical science.

We cannot but regard the third edition of Wood's Practice of Medicine as the ablest contribution of its kind to American Practical Medicine. It was proper that he who fills the chair once adorned with the eloquent enthusiasm of Rush, and more recently with the eminent abilities of Chapman should do something to show that the mantle of such prophets had descended upon shoulders worthy to wear it. It has been the good fortune of Professor Wood

to do more than this; he has shown that he stands among such prophets as Saul stood among the people of his day —taller from the shoulders upwards. Where Professor Rush revelled in the luxuriance of a prurient fancy, and the most preposterous theories, Professor Wood abounds in sound, practical sense, upon which men may fix their faith. Where Dr. Rush was all wrong, Dr. Wood is thoroughly right, and more sound philosophy may be deduced from any of the subjects treated by Professor Wood, than can be extracted from all the tomes of Dr. Rush. When, from the vantage ground now occupied by the profession, we look upon the labors of Dr. Rush, and recognize the vast influence he wielded, and the manner in which he lorded it over the intellects of men; we may safely congratulate alike the medical profession and humanity.

We cannot too earnestly commend the work of Professor Wood to the attention of medical readers. This third edition is worthy of a place in any medical library, no matter how full it may be, nor how choice the selection is.

The work is well printed, and does credit to the publishers. It may be purchased in this city at the book store of Morton & Griswold. B.

BIBLIOGRAPHICAL NOTICES.

Art. III.—*An Essay on the Yellow Fever, as it has occurred in Charleston, including its origin and progress up to the present time.* By Thos. Y. Simons, M. D., Port Physician, formerly President of the Medical Society, and Professor of Practice of Physic in the late Medical College of South Carolina, medical adviser and formerly Chairman of the Board of Health, etc. Read before the South Carolina Association, at its Anniversary Meeting, May, 1851. Charleston, S. C. 1851.

In this Essay, Dr. Simons gives, 1st. The history of yellow fever from its first appearance in Charleston to the present time; 2d. The causes which have been supposed to produce the disease; 3d. Describes the former condition of the city, its present condition, and the system of medical police adopted; 4th. Points out the peculiar character of the disease and the diagnosis between it and other fevers of that vicinity; and 5th. Gives a succinct view of the various remedial agents adopted for its cure.

Dr. Simons is a believer in the indigenous character of yellow fever. He believes that while a ship may originate the infection, it cannot propagate it except in an unhealthy atmosphere. He says:

From the statements which I have brought to your view, you will readily perceive, that while I regard it as proper to prevent, as far as practicable, the introduction of yellow fever from other sources, we should not be forgetful to remove all causes which may arise among us to produce the disease. It is a great error to conceive that

we receive all the evils of epidemics from other sources, rather than causes among ourselves. The true, and in my opinion, the wise plan, is to attend particularly to internal medical police, with a judicious care as regards introduction of it from other sources. I must in candor say, that I do not believe the yellow fever was ever produced in Charleston from importation; and this is the result of an experience of thirty years as Port Physician, and twenty-four years a member of the Board of Health, many years of which I have been its chairman.

ART. IV.—*On the Surgical Treatment of Polypi of the Larynx, and Oedema of the Glottis.* By HORACE GREEN, A. M., M. D., President of the Faculty, and Professor of the Theory and Practice of Medicine, in the New York Medical College; Member of the American Medical Association; Fellow of the College of Physicians and Surgeons, N. Y.; and Honorary Member of the Philadelphia Medical Society. New York: G. P. Putnam: 1852: pp. 121: 8vo.

The affections of which this volume treats are of so rare occurrence, that they have attracted comparatively but little attention, but they are of so fatal a character, that they deserve to be carefully investigated. Dr. Green believes that foreign growths in the opening of the air-passages have often been overlooked where they were the cause of death, and that when the attention of the profession comes to be directed to this subject, it will be found that the existence of Polypus and other excrescences in these passages, "is an occurrence taking place much more frequently than has been supposed by medical practitioners." He has collected together the well authenticated histories of this disease, which amount to only about forty cases, of which four, or one-tenth, have occurred in his own practice within the past five or six years. Two others in the same time have been observed by Professor Parker, of New York; and we think with

the author, that amongst physicians of long experience and extensive opportunities for observation, many on reading the histories of the cases here given will recall the occurrence of similar cases in their own practice.

Of all the cases referred to by Prof. Green, the result in every instance, except three, was fatal, the life of one having been saved by laryngo-tracheotomy, performed by a German surgeon, and the other two fortunate cases having been treated by our author.

The diagnosis of polypus of the larynx is obscure. Ehrmann asserts that there is but one certain sign—the discharge, by expectoration, of some portions of the tumor, as occurred in the case for which he performed the successful operation. In some cases the tumor may be seen, but neither of these evidences are afforded until the disease has advanced to a very formidable point, and in fact the patients often die before any such indications appear. Sometimes the tumors are sessile, but in most cases, they are pediculated, and then the patients may perceive their movements during respiration.

"An alteration in the sound of the voice is one of the most constant symptoms in polypus of the larynx."

This alteration may be slight, or it may amount to complete aphonia, it may occur suddenly, or may come on gradually. Dyspnea is another prominent symptom, slight, it may be, at first, but augmenting with the progress of the disease until it threatens suffocation.

The only remedy for this affection is the removal of the tumor, to detect which the patient, in cases where one is suspected, is to be placed opposite a good light, with his head properly supported; when the operator depresses the tongue with a spatula and drags it forward until the epiglottis is brought well into view. "At this moment the patient should be directed to cough forcibly, when a portion of the polypus, if it be floating in the cavity of the glottis, as is frequently the case, may be seen, momenta-

rily, over the aryteno-epiglottic border." If this should fail, an attempt must be made to feel it with the finger. Its existence once detected, the operation is thus performed: The patient is placed as before described for examination, when the tumor, if seen, is caught with the double hook, held in the left hand of the operator. If successful in this, with the probe-pointed knife in the right hand, he is to divide the pedicle of the polypus, and with the tenaculum remove it from the larynx. In case no tumor can be detected upon repeated examinations, Dr. Green recommends the application of a strong solution of nitrate of silver to the glottis, and into the larynx of the patient; and should success attend neither plan, then he urges laryngotomy as the last resort.

We must notice briefly Dr. Green's treatment of *œdema glottidis*, the fatality of which disease may be inferred from the fact, that Dr. Bayle records but one case of recovery out of seventeen, Valleix but nine out of forty, and Sestier but forty-one out of one hundred and sixty-eight cases. His medication is topical. He applies a strong solution of nitrate of silver, sixty grains to the ounce of water, to the epiglottis and lips of the glottis, and by means of a sponge, passes some of the fluid into the ventricles of the larynx. He remarks that inflammation of the larynx, of whatever grade, is aggravated by the application of a weak preparation of the nitrate. A concentrated solution, on the contrary, arrests or changes the diseased action. He adds:

"The first application with the sponge-probang should be made to the pharynx, and the top of the epiglottis; and after a delay of ten or fifteen minutes, the measure may be repeated, and the sponge, wet with the solution, be freely applied to the base of the epiglottis, and over the edematous lips of the glottis. This application should be repeated every hour, or two hours, according to the urgeney of the disease, and the effect produced by the op-

eration, and an attempt should be made each time to carry the sponge between the lips of the glottis. As the edema at the opening of the larynx subsides, this may be done, and the application of the caustic solution, be made to the interior of the glottis."

We are sure that we need not urge our readers to make themselves familiar with the contents of this volume—in our judgment one of the most valuable contribulions to practical medicine yet made in our country.

ART. IV.—*Illustrated Manual of Operative Surgery and Surgical Anatomy.* By C. BERNARD & C. HUETTE; edited, with notes and additions, by W. H. VAN BUREN, M. D., Surgeon to Bellevue Hospital; and C. E. ISAACS, M. D., Demonstrator of Anatomy in the College of Physicians and Surgeons, New York. H. Balliere: New York. 1852.

Such is the ample title of a work now in progress of publication in New York, under the superintendence of two distinguished physicians of that city, whose talents and learning eminently fit them for the enterprise. The original work was issued, a short time ago, at Paris, and has been received, every where, with great favor, on account of the extraordinary beauty and richness of its mechanical execution, which is far superior to that of any treatise on surgery that has ever appeared. Every surgical instrument, every surgical operation, and every part of surgical anatomy, is most graphically and fully illustrated; thus enabling the merest tyro in the profession to comprehend their nature and object with as much facility almost as if they were spread out before him by the hand of a master. Indeed, it is impossible to conceive of any thing more perfect in its kind. The text has been well rendered by the American editors, who have spared neither labor nor expense to make the work

worthy of the patronage of their countrymen. Here and there they have added a valuable note, elucidating some obscure or doubtful point in the original, or illustrating some operative procedure of some of our own surgeons. What greatly enhances the value of the work is the fact that all the plates are imported direct from Paris.

The *Manual* of Bernard & Huette is to be published in four parts, of which two are already before the public. We learn that one of the editors is now in Paris, to make arrangements for the speedy completion of the work. Mean while, we take much pleasure in recommending the parts already issued to the favorable consideration of our brethren, and in assuring them that they cannot obtain, anywhere, within the same compass, and for the same amount of money, so correct, beautiful, and graphic a guide to operative surgery. When completed, the work will find a place in the library of every educated physician in the country. G.

ART. V.—*The Principles and Practice of Surgery.* Illustrated by three hundred and sixteen engravings on wood. By WILLIAM PIRRIE, F. R. S. E., Regius Professor of Surgery in the Medical College and University of Aberdeen; Surgeon to the Royal Infirmary, etc., etc. Edited with additions by JOHN NEILL, M. D., Surgeon to the Pennsylvania Hospital, Demonstrator of Anatomy in the University of Pennsylvania, etc. Philadelphia: Blanchard & Lea. 1852. pp. 184, 8vo.

Lectures on the Principles and Practice of Surgery. By BRANSBY B. COOPER, F. R. S., Senior Surgeon to Guy's Hospital, etc. Phila. Blanchard & Lea. 1852. pp. 771, 8vo.

The Principles of Surgery. By JAMES MILLER, F. R. S. E, F. R. C. S. E., Author of a Treatise on the Practice of S gery; Surgeon in Ordinary to the Queen for Scotland; Surgeon in Ordinary to his Royal Highness Prince Albert, for Scotland; Professor of Surgery in the University of Edinburgh; Senior Surgeon to the Royal Infirmary,

etc., etc. Third American from the Second and Enlarged Edinburgh Edition. Illustrated by two hundred and forty engravings on wood. Revised, with additions by F. W. SARGENT, M. D., Member of the College of Physicians of Philadelphia; Author of "Minor Surgery," etc. Philadelphia: Blanchard & Lea. 1852. pp. 751, 8vo.

Here are three volumes appearing near the same time, of nearly the same size, making in the aggregate 2,306 large octavo pages, devoted to a single branch of the profession, and sent forth in the best style of the enterprising publishers, to whom American physicians are so largely indebted. Who can doubt, then, that we are a reading profession? What publishers would venture upon the issue of so many works on the same subject, if not assured that there was a demand for them? We take the fact of the rapid sale of medical books to be indubitable proof that there is a growing taste for study in the profession of our country.

Two of these works cover the same ground, and may be viewed in the light of rivals. Pirrie's, like Cooper's, consists of Lectures delivered to his students, and possesses all the qualities of a text-book. In his preface he states, that it is "for the use of his students" that it has been prepared. Both works are eminently practical, and afford that compendious view of the subject which students require in a text-book. Miller's is devoted to the *Principles of Surgery*, and is worthy of the high reputation it has attained at home and in this country. Its aim is to attract to the science of surgery more attention than it has ordinarily received. It is a philosophical work, in which the facts of surgery are combined and classified under definite laws. In this character it prefers strong claims to the favor of the profession. Our impression is that as a manual for students, Pirrie's is the best work extant; while the name and long experience of Cooper will recommend his volume to teachers, students, and practitioners.

SELECTIONS

FROM

FOREIGN AND HOME MEDICAL JOURNALS.

London Hospital.—Abscess of the Lung following Pneumonia; Death; Autopsy. (Under the care of Dr. Pereira.)—The formation of abscess in the lung as a consequence of pneumonia is considered a very rare occurrence; but it is probably still more rare to find a circumscribed accumulation of pus in the pulmonary tissue taking on a semi-gangrenous character. Such events seem in some degree to exclude each other; for the vigorous exudation indispensable for hemming in purulent matter can hardly be contemporaneous with sphacelus of the lung. However this may be, it is plain that in constitutions brought to a state of great debility by excesses, the most unusual pathological phenomena will spring up; and these being at all times instructive, we shall just, from the notes of Mr. Whitby, relate the following case:

John A. T——, aged forty, formerly a solicitor, married, and the father of four children, was admitted, January 18, 1852, under the care of Dr. Pereira. The patient is of a dark and sallow complexion, thin, weak, and exhibiting all the signs of a deteriorated constitution; his habits have been very intemperate, the chief beverage he indulged in being spirits. His health had, however, been tolerably good, until about five months before admission, when he

was seized with pain in the right side of the chest, increased by a full inspiration; his habitual cough became very troublesome, the expectoration profuse, and of an offensive odor, pretty similar to that which was noticed on his admission. No medical aid was sought until three weeks since, when rigors had set in; which the patient tried to conquer by large potations of gin-and-water. The pain in the right side of the chest was then somewhat relieved by blisters, &c., but he was soon obliged to apply for admission into the hospital.

When first seen, the patient complained of cough and abundant expectoration; his voice was hoarse, the breath very short, the countenance anxious and sallow, the bowels relaxed, but the appetite still good. The tongue was furred and moist, with an unpleasant taste in the mouth; the pulse rapid and small, and the urine abundant, but very ammoniacal.

On examination of the chest, dullness was found over the right subclavicular and mammary regions, the corresponding parts on the left side being resonant: respiratory murmur, indistinct over a great portion of the right side of the chest, both anteriorly and posteriorly, whilst it was puerile on the left. To the right of the central part of the sternum a peculiar tremulous sound is heard, which has much resemblance with the pulsation of a large vessel through a cavity containing fluid. This sound was not, however, constantly noticed, nor could it be excited by any particular position of the body. The patient was aware of this peculiar bruit; he could himself hear it; and compared it to the noise made by water bubbling from the lid of a tea-kettle. The action of the heart was accelerated, but no other irregularity was detected. As to the voice sounds, pectoriloquy was heard at the base of the right scapula. When inquiring about the patient's sensations, it was found that the right side of the chest was painful, and that the cough was extremely troublesome when he lay on his left side, the easiest posture being the slightly raised dorsal decubitus.

Dr. Pereira ordered bark and ammonia to be taken three times a day. The symptoms continued pretty well unaltered for the next few days, during which the patient took, first chlorate of potash, and subsequently small doses of sulphate of quinine and morphia. The sputa re-

mained of the same nature, the perspirations at night became very profuse, and the relaxation of the bowels could hardly be restrained by logwood and chalk. Thus the patient progressed for about three weeks, the amphoric murmur on respiration being audible over the right side of the chest, and the heart's action being accompanied by a peculiar gurgling and undulatory sound.

From the physical signs, and the nature of the symptoms which had existed previous to the patient's admission, it was assumed that he had suffered from pleuropneumonia; which had resulted in abscess of the lung, with adhesion of its walls to the parietes of the chest, besides effusion into the cavity of the pleura. Thus was a tremulous sound produced with each impulse of the heart, resembling the vibration produced by tapping a phial containing air and fluid. Twenty-six days after admission, the patient died suddenly, after expectorating a large quantity of very offensive matter.

Post-Mortem Examination.—On viewing the chest, before proceeding to the inspection of the thoracic viscera, it was observed that the right side was larger than the left, and presented a rounded appearance, with the filling up of the intercostal spaces. The thorax, when opened, was found completely filled with pus of a very offensive odor. A large abscess was situated in the right lung, the outer wall of the cavity being adherent to the pleura costalis; and running in front of the lung in a transverse direction, was a broad sinus of communication, extending to the middle mediastinum, which region might be considered as the internal boundary of the abscess. The substance of the lung contained a large quantity of mucus, mixed with air and pus, as well as small tubercles and pieces of lymph. The bronchial tubes were filled with matter similar to that contained in the abscess, and the lung was adherent, over its whole surface, to the parietes of the chest. The left organ was perfectly healthy, but the cavity of the pleura contained about five ounces of serum. The heart was pale and of lax fibre, but otherwise in a normal condition.

It is clear that certain changes in the walls of the above described abscess, or a decomposition of its secretion, must have given rise to the fœtor so characteristic of gangrene. The latter did not, however, exist in the manner

usually seen, namely, a crumbling down of the pulmonary tissue, and a dark livid color of parts. A circumstance which is finally worthy of record is, that the fœtor had existed from the commencement of the purulent secretion.—*London Lancet.*

Phlegmasia Dolens Occurring in a Female, and not Connected with the Puerperal State. By John Kelly, M. D., of Esperance, N. Y.—(Communicated for the Boston Medical and Surgical Journal.) On the 7th of August, 1847, I was called to see Miss S. Scott, in the town of Schoharie, who had been many years feeble and rather leuco-phlegmatic. She was then laboring under fever, with headache, more on the left side of the head than on the right; pulse strong and tense. I thought it not advisable to bleed at this time, but a week after I bled her to the amount of about ℥ viij. This relieved the head, and perhaps a blister which I put to the nape of the neck had some good effect. The general treatment consisted in the use of blue pill, laxatives and digitalis purpura.

Aug. 23. Found her symptoms improving, headache gone, and appetite better; yet as her pulse was not quite soft enough to suit me, I thought best to continue treatment. In the absence of her mother about this time, she walked out to the orchard, and soon after complained of a pain in the hip near the joint.

Sept. 2. Found her left leg some swollen, thigh more so; the inside of thigh excessively tender to the touch, tense, veins enlarged, with rather dark streaks and some hard lumps. The whole limb perfectly useless, giving the sense to her of great weight. The pulse, at this time, was more strong and tense than ever. The tongue had a white coat. Ordered purgatives of Ep. salts and cream of tartar, with an occasional dose of chloride of mercury and febrifuges.

4th. Pain more intense than ever. Ordered anodynes and a powder of three fourths of a grain of digitalis and eight grains cream of tartar every four hours. Local applications of infusion of poppy and hot vinegar.

7th. Found her no better. Ordered sal. ammoniac dissolved, and laudanum to the most painful parts; and to

be given every four hours, five grains cream of tartar, three fourths of a grain of opium, as she had not slept for three or four days and nights.

9th. Found her about the same. She wished to be moved often from her back to her side and vice versa. Ordered xxv. grs. calomel, crem. tart. v. grs., digitalis gr. j., every four or five hours.

10th. Calomel operated favorably. She was easier; gums slightly affected. The thigh not so much swollen, nor so tender. Ordered pill blue mass and digitalis daily, applying a wash of op. ʒ i., sal. ammoniac, ʒ jiss., camphor, ʒ jss., dissolved in spirits.

11th. Improving. Continue the same treatment.

12th. Improving. Continue same treatment, and a cathartic of cream of tartar and jalap.

A few days after the left leg had become better, the right one was also in the same way affected, though not so severely. The fever, which had subsided, came on again, and the same treatment had to be resorted to for the purpose of subduing the constitutional symptoms, which were not so severe as at the time the first leg was affected. For some ten days there was no great improvement, except the swelling subsided measurably; but the limb continued œdematous for some time, and extremely weak. Indeed she was not able to walk for six or seven weeks after she otherwise improved. Her pulse became more soft, and her appetite improved. Anodynes once or twice a-day, and a pill of socotrine aloes were continued for sometime to compose the nerves and to regulate the uterine system. After a short time her health became confirmed, and ever since she has been one of the most healthy young ladies to be found.—*Boston Medical and Surgical Journal.*

Summer Epidemics.—Formerly, physicians had considerable to say upon summer epidemics, as though they were a fixture at certain times and in certain sections of the country. It is true that there are occasional intimations of a tendency that way, and the mortality of a city increases as the summer advances; but the very general cultivation of the land in New England, the drainage of

fields in the neighborhood of villages, careful attention in locating dwellings, the facilities for procuring pure water, and the increased intelligence of the inhabitants generally, as manifested in other respects, has had something to do in overcoming an epidemical tendency. In old houses, thickly clustered together, so that the sun cannot penetrate into all the crooked by-ways in which they are situated, and in which the poor and negligent usually monopolize every apartment that will give a tolerable shelter, the most formidable types of malignant disease are liable to manifest their energy. The conservators of the public health are quite familiar with this fact, and by keeping a vigilant eye on such receptacles of filth, neglect and poverty, they are often able to prevent a sudden outbreak of disease. Practitioners of medicine, too, understand better than formerly how to meet these messengers of terror.—It is reasonable to expect, therefore, that the community will not suffer, as in times past, either in imagination or reality, from sudden developments of anomalous summer distempers, coming and going without being clearly understood beyond the destruction that marked their track. —*Ibid.*

An Eye Destroyed by a Bird-Shot.—Mr. —— was hunting birds on the 11th March, 1843, when, in order to secure their game, he took one side of the field and his brother the other, the distance between them being supposed sufficiently great not to incur any risk in firing towards each other. His brother fired, and a single shot seems to have reached Mr. ——. This passed through the cornea and lodged within the globe of the eye. There being but little pain, and several days passing without much inflammation, the patient flattered himself that the accident would not prove very serious. The pain, however, began to be acute, inflammation rapidly increased, the eye swelled out enormously, and the patient was finally relieved by excision of the cornea, which allowed the disorganized humors to escape with the shot in their midst. Recovery took place in the usual time, and a glass was substituted, which very effectually obviates the deformity.

The reporter finds in the Southern Medical and Surgical

Journal, for 1838, (vol. 2, p. 647,) several cases recorded, in which Prof. Dugas resorted to excision of the cornea for the purpose of relieving great local pain and constitutional disturbance consequent upon a disorganization of the contents of the eye. Whenever the eye is irretrievably lost and proves a source of serious annoyance, Prof. D. thinks that it should be at once emptied, both as a measure of relief and as a security against sympathetic disease in the sound organ. He has never found any bad effects from such a course.—*Southern Med. & Surg. Jour.*

Anesthetic Agents in Operations for Gunshot Wounds. By John B. Porter, M. D., Surgeon, U. States Army.—In our former paper, the case of Williamson was presented with some remarks in relation to the use of sulphuric ether for producing anæsthesia in operations in the General Hospital at Vera Cruz, in 1847. In the summer of that year, an amputation of the thigh was performed, the patient having been put under the influence of ether, in which the hemorrhage was almost uncontrollable. The blood spouted in all directions, and I have never seen an operation where it was necessary to secure so many bleeding vessels. Even after every small vessel that could be got at was secured, it was necessary to use cold water freely to suppress the general oozing of blood. At the time, I imputed the obstinate hemorrhage to the pernicious influence of the ether. In gunshot wounds anæsthetic agents are almost universally unnecessary, and are almost universally injurious. It was for this reason that they were entirely given up in the hospital at Vera Cruz.

It may be well questioned whether anæsthetics are not calculated to produce injurious effects in all important amputations; but they certainly do so in operations performed for gunshot wounds. M. Velpeau says: "Chloroform evidently depresses the nervous system, and as great prostration always exists in patients who have received gunshot wounds, it is advisable to refrain from any anæsthetic means."—*Ranking's Abstract*, 1848. Mr. Alcock refers to the cases of soldiers wounded in battle, where the excitement is such as to carry them through almost any operation. I regret that Mr. Alcock's paper is not before me. These are the cases spoken of by Mr. Guthrie:

"Soldiers in general are anxious to undergo an operation when they find it inevitable, and frequently press it before the proper time; that is, before they have sufficiently recovered the shock of the injury."—*Gunshot Wounds*, p. 232. These are the cases which require a little more time, some "encouraging words," and perhaps a little wine or brandy and water; but no anæsthetics, for the patients are already sufficiently depressed.

There are two sets of cases; in the one (Velpeau's,) the shock to the nervous system is great, from which the patient may not recover, and the use of anæsthetics would be awfully destructive; in the other class, they are unnecessary, and would prove useless and injurious. In the flap operation, they must prove more injurious than in the circular; from the fact that muscle forms almost the entire covering for the stump; and the contractility of the muscular tissue is for a time almost annihilated, to be recovered irregularly at irregular intervals. Further, after the use of these agents wounds do not heal so readily by the first intention.

M. Jobert, on the use of ether, states that the local inflammation has proved less, and that union by the first intention has been prevented. I am able to bear testimony to the correctness of M. Jobert's statement.—*American Journal.*

Chloride of Sodium in Intermittent Fever.—Wishing, then, to experiment with salt, a few cases of intermittent fever (old stagers), contracted in Algiers were selected as subjects. Behold, then, Piorry at the bedside. The patient asserts that he contracted the fever and ague several years since in Africa; that he has frequently been cured; but that the disease has constantly reappeared at the end of fifteen days or one month at farthest. The type of the fever is tertian. The spleen is percussed and found to be abnormally dull throughout its whole extent; the entire splenic region is sensitive upon percussion, particularly over the dullest points; and each blow is accompanied by marked contortions of the countenance. This sensibility extends but little beyond the region of dullness, which last occupies an extent of fifty-three lines, measuring in

the direction indicated above. To this patient a drachm of salicine is administered without producing any change in the dimensions of the spleen. A few minutes subsequently, half an ounce of salt mixed with a cup of soup is given, and upon carefully percussing the splenic region at the end of four minutes, this organ is found diminished one inch, from above downwards. The next day the spleen is found to be of the same size, but upon the administration of a second dose of salt, it suddenly contracts and measures nearly three quarters of an inch less than yesterday. The resonance throughout the entire organ has increased while the sensibility has diminished. The succeeding day, the attack of fever is very slight, and upon giving a third dose, the disease does not return; and when seen six weeks subsequently, the patient is still free from his African enemy. Thus we see that a diminution of twenty four lines in the length of the spleen was the result of the medicament, the fever being cured more effectually than ever before; *i. e.*, the patient had remained free from all relapse for the space of six weeks; one month having previously been the longest period of immunity.

We have the notes of seven cases of well-marked intermittent fever, in all of which the administration of the chloride of sodium was followed by rapid decrease in the volume of the spleen and cure of the febrile symptoms. We also have the record of three cases in which salt was unsuccessfully used; in one of these, the sulphate of quinine effected a cure; in a second it too failed, while in the third it was not tried. These were all well-marked cases of intermittent fever, such as would pass muster in any of our own malarious districts.

Let it be remembered that most of the fever and ague met with in the Parisian hospitals, is of long standing, and imported from the malarious districts of Algiers, which generate a form of the disease even worse than that found amid the marshes on the banks of the famed Maumee; that these cases have been treated again and again, have been cured now by the sulphate of quinine, now by arsenic, but only to reappear upon the slightest exposure or imprudence; in short, to recur as only *the shakes* can recur.

We witnessed many of the experiments of M. Piorry,

and in the great majority of them, the fever yielded to salt quite as readily as to the salts of quinia. And as to the theory of M. Piorry, the spleen diminished under the use of the remedy, *pari passu*, with the febrile symptoms, in every case where the disease was cured, proving that this organ really shows the influence of remedies over this class of fevers—that it is, as it were, a febro-barometer —for the diminution of the spleen is a constant phenomenon accompanying the cure of the disease, whatever be the curative agent employed.

M. Piorry's method of administering the chloride of sodium is, to give half an ounce in a cup of thin soup during the apyrexia and fasting. It usually agrees with the stomach perfectly well, but in some few cases we have seen it excite vomiting and diarrhea. Three doses commonly suffice to effect a cure, the first two to be taken on succeeding days, and the third after an interval of one day. Should the spleen be undiminished in volume by the first dose, we may be sure that the remedy will not cure the disease; and the same is true of all the antiperiodics. Excepting in rare cases, the diminution of the spleen occurs immediately upon the administration of the remedy (salt or sulph. quinine,) and may frequently be detected within one minute, after which the organ remains stationary until a second dose of the medicament be administered.

Is the chloride of sodium as efficient an antiperiodic as the sulphate of quinine? Are the cures effected by the one as permanent as those effected by the other? The first question can only be answered by those possessing a larger field of observation than the writer. May we not hope for a solution from those of our profession who observe the disease too largely either for comfort or pleasure? In regard to the permanency of the cures, we apprehend there is not much difference, be the medication what it may; for relapses are only too common after the greatest care and most patient attention.

Should the discovery prove as useful and applicable as it promises, the benefit accruing from it will be immense. If it be capable of taking the place of the sulphate of quinine in the majority, or even in one half the cases of intermittent fever, therapeutics will be largely the gainer.—*Ibid.*

On the Bite of the Rattle Snake.—By Lt. J. C. Woodruff. Wednesday, Sept. 17, 1851. This morning Lieut. J. F. Parke, Top'l Engineers, U. S. Army, and I, were walking out to procure some specimens of birds, and when about two miles from the Pueblo, I came within a few inches of treading upon a rattlesnake who immediately coiled himself up and got ready to strike; jumping back, I drew out my ramrod and struck him over the back with sufficient force to break it. Being a fine specimen I wished to preserve it without further injury, when placing my gun upon its head, seizing it, as I thought, immediately back of the head, I picked him up, but unfortunately I had too long a hold, when he threw round his head and buried his fang in the side of the index finger of my left hand, about the middle of the first phalanx. The pain was intense, momentarily producing, as it were, a severe shock, and accompanied with much nausea. I immediately commenced sucking the wound, at the same time got Lieut. Parke to to apply a ligature round the finger to prevent the too rapid absorption of the poison. I then scarified it freely and continued sucking until I returned to camp.

A man that was with us at the time I sent immediately back to get some aqua ammonia fort, and meet us on the road, which he did when we were about three-fourths of a mile from the town. I applied it immediately to the wound. Mr. Kern hearing what had happened, returned with him, and he wished me to try, as he said, the *Western Remedy*, that is to say, get drunk. This I had often heard of, and I was determined to try its efficacy. He was supplied with a bottle of whisky, which I immediately commenced drinking; by the time I arrived at the Pueblo, I had drank half a pint. Already the glands in my axilla were getting sore and painful. Took some ammonia internally, scarified my finger freely and held it in a basin of warm water, which caused it to bleed freely. Then commenced drinking *brandy*, and at the same time held my finger in a cup of ammonia. It took one quart of fourth proof brandy and half a pint of whisky (enough to have killed a man under ordinary circumstances) to produce intoxication, which only lasted about four hours. During my intoxication I vomited freely; soon after my recovery from this state I removed the ligature and applied a large poultice of Pulv. Sem. Lini. That afternoon I took am-

monia internally and some pills composed of Mass Hydrarg. et Collocynth Comp., to act as a cathartic. In the evening the pain in the axilla and finger was very severe; took Pulv. Doveri, grs. x.

Thursday 18th. I passed a restless night without sleep, although during the night I took at least Pulv. Opii, grs. iv. This morning the pain in my finger is intense, and a well-marked line of inflammation extends along the arm to the axilla. I had the entire arm and hand painted with Tinct. Iodine, and the flaxseed poultice renewed, commenced taking a solution of Potassii Iodidi as an alterative. The pills not having operated I took Pulv. Seidlitz, which had the desired effect. Diet, boiled rice. Several times to-day I tried to walk across the room, but each time would be seized with nausea and commenced vomiting. Took at bed time Pulv. Doveri, grs. x.

Friday 19th. I rested pretty well last night, but this morning my hand, arm, and the glands in the axilla, are much swollen and very painful.

Repeated Tinct. Iodine. Diet, boiled farina. Took on retiring, Pulv. Doveri, grs. x.

Saturday 20th. Passed a tolerable night, but my back is getting very sore, as the blankets on the stone floor make rather a hard bed. This morning the pain is very great, and the swelling down my left side as far as my hip. Renewed Tinct. Iodine. I am still attacked with nausea and vomiting on my attempting to walk.

I removed the skin from off my finger, and it discharged freely a watery sanguineous fluid without smell. The nail is becoming loose. The broad red line following the course of the lymphatic, is now filled with a yellowish serum. The point where the fang entered, for three-eighths of an inch in diameter, is of a dark brown color. Renewed the poultice. At bed time took Mass Hydrarg. grs. v. Pulv. Doveri, grs. x. Continued Potassii Iodidi. Diet the same.

Sunday 21st. Passed a restless night, being much troubled with colic; took Magnesia Calc. et Spts. Menth Pip., which relieved me, and not having my bowels open took Pulv. Seidlitz, which had the desired effect. Hand much swollen and filled with serum. Diet as usual.

Monday 22d. Passed a comfortable night. The swelling has left my side and arm, but little remains in the

hand. I can now walk a few yards without being seized with nausea; have been sitting up the most of the day. Continued Potassii Iodidi. Diet, mutton broth and farina.

Tuesday 23d. I awoke this morning much improved, the swelling and pain having left, with the exception of the finger, the first and second joint of which does not pesent a healthy appearance, the palmar surface having the appearance of gangrene, but the discharge is thin and watery, without smell. The granulations do not present a healthy appearance, they are rough, and many of them look as if they were sprinkled with yellow ochre. The nail is quite loose. Continued Potassii Iodidi. Diet, mutton broth, with a little of the meat.

Wednesday 24th. This day we commenced our march. I placed my hand in a sling and mounted my mule; found myself rather weak, and the mule hard to manage with but one hand; the sun was rather hot, this, with the jolting of the animal caused me to suffer considerable pain; fortunately for me, after going six miles, we encamped. I removed the nail. From this time on the finger gradually improved. I continued renewing the poultice daily until the last of October. In the meantime there was a large slough which gradually came away and left the last phalanx exposed in two places. The granulations required occasionally the application of nitrate of silver. After this I made use of dressings of Cer. Simplex. Continued carrying my hand in a sling until the middle of November. A new nail commenced growing and a sinus remained open in the end of the finger; upon the introduction of the probe into the latter, the bone could be felt quite rough. A discharge from this kept up until about the 7th of February, when I removed the exfoliation of the end of the phalanx, showing evidently that the fang had entered the periosteum. Soon after this the sinus closed, leaving the finger in a deformed state, anchylosis having taken place in the first joint. The circulation is very imperfect, one of the arteries being destroyed, which renders it very susceptible of cold. The insertion of the flexor muscle has also been destroyed.

I have heard of a number of instances of rattlesnake bites, in all of which the patient recovered if they succeeded in producing intoxication.

Dr. Fischer C. Smith, of this city, accompanied Capt. French, A. Q. M. U. S. Army, to El Paso last year, and on their return one of the teamsters was bitten by a rattlesnake; he gave him nothing but whisky, and in three days after he was driving his team. In this case it took three pints of whisky to produce intoxication.

Should this brief extract be of any service to you it is at your disposal.—*Buffalo Med. Jour.*

Chloroform in Obstruction of the Bowels from Spasms. By D. J. Cain, M. D.—Every physician meets, in the course of his practice, with cases of obstruction of the intestines, which has come on gradually or suddenly, generally, from some cause of irritation existing in them. The obstruction in these cases consists in spasmodic contraction of a portion, or of portions, of the intestines, generally the small. The plan of treatment which I formerly pursued was, to cease all attempts at forcing a passage by means of cathartics, if one or two brisk cathartics failed at the commencement, and to resort to opium freely, enemata of warm water, melted lard or butter, sweet oil, etc., the warm bath, fomentations to the abdomen, and other means of inducing relaxation. For more than two years past, I have used chloroform, as a more powerful agent than opium and its preparations, and as more certain in relaxing the muscular system. The chloroform, administered in inhalation, soon produces a greater or less degree of resolution, and, taking advantage of the relaxation thus effected, I give enemata, either stimulating, mucilaginous, or oily, which in a short time bring away fecal matter. The inhalation may be repeated as frequently as, in the judgment of the physician, the case demands.

Chloroform possesses the immense advantage over opium, of relieving effectually and promptly the pain, and in not leaving the bowels in a constricted state, the sedative effect soon passing off.

Seven cases have been thus treathd by me, with highly satisfactory results. In one case, only, have I experienced any difficulty in inducing the requisite degree of relaxation of the bowels. The subject of this case was very

slightly susceptible of its influence; but the pain was completely relieved by frequent inhalations, and the obstruction gradually overcome.—*Charleston Med. Jour.*

Nitric Acid in Hooping-Cough and Asthma.—Dr. F. C. T. Arnoldt recommends (*Canada Medical Journal, June,* 1852) the nitric acid as a powerful remedy in hooping-cough and asthma.

"In hooping-cough," he says, "at whatever age, whether it be a child at the breast or a full grown adult, I administer nitric acid in solution, as strong as lemon juice, sweetened, *ad libitum.* I have given to a child of two years of age as much as one drachm and a half of concentrated nitric acid, in the above manner per diem, and I have never known the disease to resist its use beyond three weeks. In one instance, that of a child at the breast, only seven months old, the disease disappeared within eight days. In another instance, of a young lady fifteen years of age, the paroxysms were subdued within the first twenty-four hours, and the disease disappeared within ten days. Again, in the cases of two boys about ten years of age, living at a great distance from one another, who had had the cough for several weeks, and to such a violent degree, that both of them had the circumference of their eyes ecchymosed, as though they had been pommelled in pugilistic combats, the acid acted positively like a miracle. A medical *confrère* of mine had four of his children severely affected with the same disease in the middle of winter; and, although they had to be kept indoors, owing to the inclemency of the weather, they were nevertheless all perfectly cured within three weeks. I might go on to cite a hundred similar instances, but these, I am satisfied, will prove sufficient to induce the profession to adopt this treatment. As regards asthma, the use of nitric acid has proved not only in my own practice, but in that of others who have adopted it, truly marvellous."

[It will be remembered that Dr. Hopkins, of Bethel, Georgia, in a communication published in the number of this journal for October, 1850, page 549, extols the powers of nitric acid in asthma, and relates five cases in which he successfully employed it.]

Lemon Juice in Acute Rheumatism. By T. D. Lee, M. D., of New York.—*Case* 1.—William Coleman, fifty-one years of age, has had frequent and severe attacks of acute rheumatism for the last fourteen years.

April 30th, 1852.—Patient has not been able to remove from his bed for the last four weeks; and he is unable to sleep by night on account of acute pain; his hands, knees, and feet are very much swollen; and he is constantly growing worse. Patient has heretofore had attacks which have lasted three or four months. At 8 o'clock, P. M., patient commenced taking lemon juice, fresh from lemons; a tablespoonful in twice the quantity of cold water with a little sugar, every hour.

May 1st, 9 o'clock, A. M.,—Patient sitting up endeavoring to shave himself. Says he feels better in every respect, and has slept quietly for four hours, which he has not done before for four weeks. Has taken six ounces of the juice. Same regimen to be continued as before.

May 2d, 9 o'clock, A. M.—Patient has slept well during the night, and is sitting up eating his breakfast. Six ounces taken. Says "lemon juice goes right to the spot."

May 3d, A. M.—Free from pain, and only complains of soreness in his feet. Same quantity as before.

May 4th.—Still improving, having taken six ounces regularly.

May 5th.—Comfortable, and taking the same quantity of lemon.

May 6th.—Doing well, still taking the same quantity of lemon, having no disposition to leave it off.

May 7th.—Patient is able to walk about his room, and does not speak of any pain. Lemon continued.

May 8th.—Patient complains only of weakness in walking. Lemon juice discontinued, not having disagreed with the stomach or bowels in the slightest degree.

May 9th.—Patient sleeps well by night; has a good appetite, and is fast gaining strength.

May 10th.—Patient is only waiting for fair weather to go out.

Case 2.—Mrs. P., seventeen years of age, has had frequent attacks of acute rheumatism for the last seven years. She is affected with enlargement of the heart. Patient is confined to her bed, and is unable to move or

sleep on account of the severe pain she suffers; and wishes to have something prescribed to make her sleep. Patient was advised to try lemon juice for 24 hours, before using any other remedy; to this she assented. Remedy commenced at 10 A. M., as in case 1st.

May 2d.—Patient has slept well, and has much less pain and soreness.

May 3d, 4th and 5th.—Improving. Continue lemon as before.

May 6th.—Free from all appearance of the disease. Lemon discontinued.

May 7th. In this case, as in Case 1st, there appears to be a flexibility of the joints, unusual in recoveries after other modes of treatment.

In a third case similar to Case 1st, the lemon appeared to be equally beneficial. In two other cases, one subacute, the other chronic in type, the lemon juice had an equally good effect.—*N. Y. Med. Jour.*

Fracture of the Os Brachii at the insertion of the Deltoid Muscle in a Colored Preacher while in the act of Gesticulating. By W. Parker, M. D., Professor of Surgery in the College of Physicians and Surgeons, New York.—I was called in consultation with Dr. Sargeant, in Jnly, 1844, in the case of Mr. R. Jackson, a colored minister of the Methodist denomination, who was suffering from a fracture of his right arm. He was of a stout build, very muscular and well developed, thirty-eight years of age, having no hereditary predispositions, and had never previously suffered a fracture of any of his limbs. There was no circumstance connected with his history giving evidence of unnatural fragility of his bones. The accident occurred while warmly engaged in an exhortation in a religious meeting, and upon making a violent gesticulation. On examination, the right humerus was found broken at the attachment of the deltoid. Union took place in the usual time.—*Ibid.*

Croup—Compound Tincture of Chloroform as a Remedy. —(*Boston Soc. for Med. Improvement.*)—Dr. Strong was consulted, late on Sunday evening, in reference to a case of alleged croup. The parents had been familiar with the disease. After inquiring respecting the symptoms, it was thought best to send directions for the treatment, with the understanding that it should be tried, and if without any relief, Dr. S. was to be summoned in the night—otherwise, to visit next morning. The child is about two years old. The prescription for the night was hydrargyri subsulphatis ℈ss.; pulvis ipecacuanhæ ℈j.; pulvis ipecac. et opii, gr. vj.; misce et div. in chart. no. vj.; one powder to be given every fifteen minutes until free vomiting was induced; the feet to be first placed in warm water; and in case of no relief, or if there should be a temporary relief, and then a return of symptoms, the same to be repeated after an interval of two hours. Dr. S. saw the child the next forenoon, and learned that the emetic had been taken, with some temporary relief; the child, at his visit, had quick, laborious breathing, with a croupy cough, aggravated from time to time, and the peculiar dry, sonorous character of the respiration incident to the worst forms of croup; the countenance at times livid. The throat, carefully examined by sunlight, showed white lymph upon both tonsils and over the fauces. Having treated other cases like this without avail, Dr. S. was strongly impressed with the idea that the child would die, and so expressed himself. A cathartic was prescribed, the bowels being confined; and compound tincture of chloroform was freely applied, by cloths saturated with it, to the neck, and the fauces and tonsils were wetted with it by means of a linen "swab." The cloths were re-moistened and applied as soon as they became dry; and this was persevered in through the day, the applications and swabbing being attended to perhaps as often as once an hour. At a visit made that evening, the same treatment was directed for the ensuing night, with the addition of a grain of Dover's powder, which was to be repeated every two to four hours, as the symptoms were, or were not urgent.

Next morning the child was better, there had been no attacks of severe dyspnœa; and there were intervals when the breathing was nearly natural. The same treat-

ment was still continued; the application to the throat to be made at longer intervals; in the evening of this day all the croupy symptoms were gone, and the child recovered rapidly. Auscultation detected evidences of bronchitis.

The compound tincture of chloroform has been used by Dr. S. in incipient inflammations—as in bubo, in inflamed glands of the neck, paronychia, and deep-seated inflammation of the hand; also, in inflamed and ulcerated gums; and, so far, with most marked advantage.—*Amer. Jour. Med. Sci.*

Operation of the Tracheotomy in an Epileptic. By Dr. Neill.—The views of Marshall Hall, which have lately appeared in the English journals upon the subject of epilepsy, have probably fallen under the notice of most of the Fellows of the College.

Every investigation of a malady so distressing, and of which so little has been known of its pathology, must be hailed with pleasure by every practitioner of medicine. Especially will this be so, when so high an authority upon affections of the nervous system advances views not only as to the nature of the disease, but also practical deductions of the highest importance.

Dr. Marshall Hall says: "This question of the application of tracheotomy in the preventive treatment of epileptic convulsion, is one involving high principles in physiology.

"As I have stated, I believe few will hesitate to perform the operation of tracheotomy, as the present remedy, when there is, from apoplectic laryngismus, imminent danger to life. But the question remains—are we justified in performing this operation in cases of epileptic and other convulsions, as a preventive of future evil? Are the somewhat remoter dangers to mind, and limb, and life, and the hope that whilst the faculties are spared the patient may be rescued from the susceptibility to the attacks, the *dignus vindice nodus*, a sufficient motive for adopting this measure in its more continuous mode of a tube worn in the trachea. After having witnessed the dire circumstances and effects of the frightful maladies more than any

man, of epilepsy especially, I unhesitatingly say, yes! I regard the melancholy condition of the patient as justifying the heroic remedy. The case may be violent and frightful *in any degree.* In what precise case tracheotomy is justifiable I do not pretend to determine. It is a matter of pure *moral* calculation and choice in regard to the terrors of the malady on one hand, and of the remedy on the other. Epilepsy may occur in the slightest form of mere transient oblivion, and it may occur in the gravest form of sudden and violent convulsion, dashing the patient to the ground, into the fire, or into the water, and followed by coma or apoplexy, delirium or mania, paralysis, amentia.

"The former of these attacks may be designated the *epilepsia mitior.* Itcomprises all that is short of laryngismus, affections of the senses, as muscæ, tinnitus, the odor of musk, aura, vertigo, oblivion, confusion, loss of consciousness, nutatio, falling, various spasmodic affections of the face, the eyes, the extremities.

Then comes laryngismus, laryngeal dyspnœa, perhaps perfect closure of the larynx, with violent efforts of expiration. This with all the other links of the dreadful chain constitute the *epilepsia gravior;* all that is on *this* side of the laryngismus must be unaffected by the operation of tracheotomy; all that is on *that* side of this laryngismus will, I trust and believe, be prevented by its efficient institution. By tracheotomy, the *epilepsia gravior* or the 'grand mal,' is converted into the *epilepsia mitior*, or the 'petit mal.' If this, my hope, be realized, I shall deem the event a great victory achieved by physiology or theory over mere observation, and especially by that of the diastaltic nervous system, of which it is an application.

"I may now observe, in conclusion, that I have on several occasions stated that, if tracheotomy were performed, and a tube worn in the trachea, the epileptic, the puerperal, or the infantile convulsion would be prevented, with its dire effects."

In accordance with these views I operated upon a patient of Dr. Shelmerdine, in Spring Garden, under whose care he had been for about one year, and who had tried all the ordinary modes of treatment. The following are the particulars of his case:—

John Blume, aged twenty-nine, of five feet eleven

inches in height, and weight about one hundred and sixty pounds. His appearance was healthy, and he had no deformity of the throat.

His first fit occurred nine years ago, and was not referable to any particular cause by his family. He was not subject to them in childhood, although his brother had died of epilepsy.

The frequency of the paroxysms gradually increased, and for the last year he has been unable to attend to any business.

His mind has been so affected by the disease that he has frequently mistaken his way home, and often gone into the neighbors' houses for his own.

His mother and wife informed me that during the last six months he would have an attack at least every other day, but occasionally would have as many as fifteen or twenty during the day.

Life had become a burden to him, and he feared to leave his home.

His physician tells me that on the first occasion of his being called to him, he was laboring under most severe congestion of the face and neck, producing great lividity and complete insensibility; and that, in all of the subsequent attacks, difficulty of breathing seemed to be prominent.

The patient himself remarked to me that, immediately preceding his attacks, he frequently experienced a sense of constriction about his windpipe; and his friends and family confirmed the idea, that the severity of the attack was proportionate to the difficulty of breathing.

When the operation was proposed, and its nature explained to him, he was anxious for its performance, and had great expectations of its relieving him.

The operation was performed on the 11th of March last, in the presence of Drs. Shelmerdine, Marshall Paul, and Hollingsworth.

His neck was long and well adapted for the operation. The incisions were made in the usual way, and the only points worthy of remark were—that the sterno-hyoid muscles, from frequent spasmodic contractions, were thicker than usual; and that the isthmus of the thyroid gland was so large and broad as to cover the first three rings of the trachea.

The hemorrhage was not so troublesome as might have been expected; care was taken to tie the inferior thyroid vein, and no irregular artery was met with after the trachea was exposed. A piece of about three lines in breadth, was removed from the middle of the fourth ring of the trachea, and the fifth ring also was divided in order to accommodate more accurately the tube which had been provided, which was of the ordinary form of the instrument shops.

The introduction of the tube produced but little irritation and coughing, his voice was not in the least affected; but the trachea was smaller than usual, and the wound becoming so very deep after the division, that I had constructed tubes of various angles and length corresponding with the depth of the wound. (Specimens of the tubes were exhibited to the College.)

He slept but little the first few nights after the operation, and seemed unwilling at first to trust himself in a recumbent position, but as the wound healed around the tube he became comfortable, and had nothing like a return of his complaint until the *thirteenth* day after the operation, which tendency to an attack he attributed to his removal of the tube; he had taken a slight cold, which made the tube disagreeable on that day, and he thought he would risk the night without it. The spasm was slight, and he did not lose his consciousness. About *two weeks* after this he was threatened with an attack of which he was conscious, and mentioned the fact to his mother, who immediately removed a temporary plug which he introduced in the orifice of his tube to prevent a whistling noise accompanying respiratory movements. Upon the removal of the plug, the symptoms disappeared, his breathing was comfortable, and he felt much encouraged. He began to appreciate the object of the operation, and fully believed that the means to mitigate the severity of his attacks was the removal of the plug, and that the disease was under his own control.

He made arrangements to renew his business, walked about the streets in the confidence and consciousness of a strength of mind and purpose which he had not experienced for a long period.

Unfortunately for him, however, he was again seized on the evening of the 2d of May, with symptoms of another

attack. His physician was sent for, who removed the tube and cleansed it; after it was replaced the patient felt easier, but was not completely relieved. In the middle of the night he had a most violent attack, and died almost instantaneously. His physician was not with him when he died, and the family would not permit a *post-mortem* examination of any part of his body but his throat. Dr. Shelmerdine merely examined the cicatrix around the wound and trachea. The parts had consolidated around the tube, and the trachea was perfectly healthy.

I report this case to the College in order that they may form their own judgment upon the theory and the treatment of Marshall Hall. Few cases have as yet been reported where this operation has been performed, and I believe that this is the first case in this country in which the trachea has been opened, and a tube worn, in order to mitigate, if not prevent, attacks of epilepsy.

And, although this patient died, I still think favorably of the operation, and under the same circumstances would perform it again. His death was in no way attributable to the operation, and had not the operation been performed it might have occurred at a still earlier period. I regard the mitigation of the attacks with which he was once threatened, and moderation of the symptoms, as more satisfactory than if there had been no approach of an attack, for then the entire absence of the complaint might have been attributed to the shock made upon the system by the operation; and this operation would have demonstrated nothing more than tying the carotid artery, after which, and other violent shocks, patients have been free from attacks for a long period.—*Transactions of the College of Physicians of Philadelphia.*

New Mode of Reducing Strangulated Hernia.—Dr. Wise makes the following interesting communication in a letter addressed to Professor Syme:—

"The following are the particulars I promised to send you, regarding a new method of reducing strangulated hernia. While I had charge of an hospital in India, an elderly man was brought to it with a strangulated inguinal hernia. After in vain employing the usual means of

reduction, I was preparing to liberate the gut with a knife, when a Mussulman gentleman suggested that the following method should be first tried, as he had seen it successful. As it appeared most simple and effective, I at once proceeded to try it. The patient was placed upon a table, and a long sheet, folded several times on itself, was carried round the lower part of the abdomen of the patient, was twisted on itself in front, and again on the sides, so as to enable an assistant, standing on each side of the patient, to hold the extremities of the sheet, and to pull them gently upwards, or towards the patient's head, while a third assistant held the feet steady, and the surgeon used the taxis.

"As the gut immediately above the strangulated portion was superficial and distended with air and liquid, it was drawn upwards with considerable force from the hernial sac, which was assisted by the surgeon using the taxis; when the strangulated portion was immediately reduced.

"This simple method may, in a very large proportion of cases, be employed with perfect safety and at an early period, before inflammation and thickening has complicated and increased so much the danger of the operation, which is thus rendered unnecessary."—*London Journal of Medicine.*

Starch in Cutaneous Diseases.—M. Cazenave has lately employed, largely, powdered starch, pure or mixed with oxide of zinc or camphor, in various diseases of skin. In acute eczema, impetigo, herpes, acne rosacea, after washing the parts with a weak alkaline solution, and well drying them, some of the following powder is sprinkled, viz: Oxide of zinc, one part; starch powder, fifteen parts. In prurigo of the axillæ, the anus, or the genitals, a quarter part of camphor is added.—*Med. Times & Gazette,* May 22, 1852, from *L'Union Medicale.*

THE WESTERN JOURNAL.

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LOUISVILLE, SEPTEMBER 1, 1852.

UNIVERSITY OF LOUISVILLE.

It will be seen from the advertisement on the cover of our Journal, that the preliminary course of Lectures in the University of Louisville, will commence on the first Monday of October. We have remarked with satisfaction that the preliminary term in this school has been attended every year by growing numbers, and by this circumstance as well as others we have been inclined, more and more, to the extension of the regular term. Our belief is, that an announcement by all the schools to the effect that a change to five months had been made, would be well received by the profession; and we are convinced that it would be better to begin the course a month earlier in autumn than to continue it into the spring. We hope in a few years to see this step taken by all the schools. Whatever we may ultimately attain to, we doubt much if a term of six months would now be acceptable to students; but we think we cannot be mistaken in the opinion that they would generally approve of an extension of the term to five months. The Faculty of the University of Louisville regard the course given in October as virtually such an extension.

PURE MEDICINES.

In a recent number of this journal, we noticed the fact that the Faculty of the Medical Department of the University of Louisville had

sent out very liberal orders to Paris, and elsewhere, for preparations designed to illustrate the teachings of the several chairs of Anatomy, Physiology, Surgery, and Obstetrics, and that very valuable and extensive additions had already been made to the cabinet of Materia Medica, from the establishment of Philip Schieffelen, Haines & Co., Druggists and Chemists, New York. We omitted, at the time, to notice as fully as we desired, the great excellence of all the articles furnished by this house, for we really think that the zeal and enterprise which these gentlemen have manifested, in regard to the proper preparation and purity of medicines, are worthy of special notice, and special commendation.

The practice of medicine depends for successful results, upon the harmonious co-operation of a variety of circumstances, and when this harmony is disturbed, it is liable to prove more injurious than beneficial. Directed by principles deemed correct, as far as scientific inquiry and experience have been able to ascertain, and plentifully supplied with the implements most essential, and approved in its prosecution, it still often falls short of its beneficent purposes, and encounters mortification and defeat. The reasonable mind views a certain amount of failure with composure, for it appreciates the fallibility of human power, and knows that limits are placed, by the laws of nature, to the successful exertion of its efforts; but when the causes of failure are such as need not, and should not exist, the most lenient disposition feels constrained to condemn.

Among the many causes of the failure of curative medicine, there is one which we hope ere long to see entirely obviated by the efforts of such men as Philip Schieffelen, Haines & Co., and others who have given themseves earnestly and in good faith, to the work; we allude to the sale and use of imperfect and adulterated medicines. Very few of the medical profession, and fewer still of the general public had any adequate conception, some years ago, of the extent to which the manufacture and sale of spurious and adulterated medicines was carried, in this country and elsewhere; some startling developments made by a few curious and reliable inquirers into the matter led to the enactment, by Congress, of an inspection law, which, in its searching execution, has unfolded a most monstrous and appalling condition of things in reference to the traffic in medicines, and at the same time, has done very much to ameliorate this condition. Much, however, yet remains to be done; much that can only be done by the exercise of professional vigilance, and the proper direction of professional patronage. This vigilance

should look to the character and responsibility of every house concerned in the preparation, purchase and sale of medicines, and this patronage should be extended only to such as can produce unequivocal testimonials of their claims to confidence. There are a great many persons engaged in vending medicins who are wholly destitute of the necessary preparatory education, and who have embarked in the business, simply because they have considered it a profitable investment of capital, trusting for success to shrewd dealing, large profits, chance, and the ignorance of their customers. Many succeed, and succeed very handsomely, and by their success, lure others no better than themselves into the business; but their success is purchased at the public expense, and by the infliction of incalculable damage upon all who fall within the range of their influence. This should not, and would not be if medical men, sustained by public authority, would determine to prevent it, by drawing a broad and distinct line between the competent, and educated, and progressive apothecary, who engages in the business in a scientific way, and the mere speculator, who has taken it up as a profitable trade. In the former of these classes we have reason to think that Philip Schieffelen, Haines & Co., deserve to be ranked, and therefore, have thought proper to notice them somewhat emphatically, disclaiming, at the same time, any special or exclusive partiality for them, beyond that which we feel for all who can present equal claims to the confidence of the profession.

R.

HEALTH OF LOUISVILLE.

The health of the city has improved since we penned our remarks on the subject in our last number, and may now be pronounced excellent. Cholera, which has visited a number of the smaller towns in the State, seems to have disappeared wholly from Louisville, and dysentery which was prevalant in June and July, has so far abated, that we now rarely hear of a case. The tendency to fever seems also to be less than it was a month ago. Copious and timely showers visited the city and surrounding country during the month of August, and have been followed by a mitigation of the intense heat.

SULPHUR WATER IN STONE IN THE BLADDER.

We received a letter about a year since from our friend Dr. Debow, mentioning some remarkable cures of calculous diseases, said to have been effected by the *Red Sulphur Spring*, of Macon county, Tenn. It occurred to us that it would be premature to make public the communication at the time, and accordingly we requested the doctor to continue his observations, and procure additional facts confirmatory of the alleged powers of the water, or the reverse. We had the pleasure of receiving the following letter from him a few days since, and although not written with reference to publication we believe he will excuse us for giving it to our readers. If the accounts given him are exaggerated, the true merits of the water will be the sooner ascertained by thus publicly calling attention to it.

—

HARTSVILLE, August 27, 1852.

Professor Yandell:—Some months ago I addressed you a letter, on the subject of the probable efficacy of the Red Sulphur Spring Water, in certain forms of disease, but especially in regard to its almost marvellous effects upon such as are afflicted with stone in the bladder. I gave you, in my letter, the history of some cases that have come within my knowledge, and asked of you, how I could most certainly arrive at the fact, that the water did possess the solvent properties attributed to it. I have not had an opportunity fully to carry out your suggestions, owing to my own business preventing me from attending the spring as I had intended. I have, however, seen and heard enough to confirm me in the opinion that the water does, by some means, cause those afflicted with gravel or stone, to throw off quantities of detritus, after using it, so that though suffering intensely at the time of visiting the spring, they have in a few weeks left it either entirely cured, or very greatly relieved. Nor have I known one thus afflicted, who has not been materially benefitted by a stay of a week or two at the spring. I received a letter from a friend at the spring a few days ago, in answer to one from me, making inquiry in regard to the number of persons attending the spring this season, affected with gravel or stone, and with what benefit, and am gratified to be able to say to you, that each and every one, amounting to some seventeen, has been cured, or much benefitted by a stay of from two to four weeks. In confirmation of the above, I will state that I was at the spring sometime about the first of July last, and that I deposited in a large vial, 6 oz., two small stones—one of the magnesian phosphate, and

the other oxalate of lime, or mulberry calculus. I submitted both of them to the same treatment, to see what, if any, effect would in course of time be wrought upon them. Fresh water was supplied every morning, and no friction employed except such as would necessarily arise from pouring out and pouring in the water. In thirty-five days the magnesian phosphate was entirely dissolved; and in sixty-one days, the oxalate was reduced to a fine powder. A similar result was had last year, which I intended to communicate to you if I did not.

With these facts before us, are we not warranted in saying, that the water does possess solvent properties, and that such as are afflicted with the disease alluded to, would do well to avail themselves of so easy and so effective a remedy?

The spring has been greatly crowded this season. Persons from a great distance have visited it, and in every instance, as I am informed, have left well pleased with the water, and its effects upon them as well as with the proprietors who are worthy gentlemen.

I may be deceived in this matter, but I think I am not. I have not spoken of any other than cases of stone, but I am fully persuaded that the water is equally effectual in other forms of disease, particularly in scrofulous affections and diseases of the skin, &c. &c.

Very Respectfully, ARCHIBALD M. DEBOW.

FLINT ON CONTINUED FEVER.

Through the *Buffalo Medical Journal* we had been made acquainted with most of the matter of this work, which consists of three reports on Continued Fever, based on the analysis of one hundred and sixty-four cases treated by Professor Flint at the Buffalo Hospital of the Sisters of Charity. The labor of preparing these reports was undertaken at the request of the New York State Medical Society, with particular reference to the question of the identity or non-identity of Typhus and Typhoid fever. The cases observed by the author since the assignment to him of this duty, he has subjected to numerical analysis, and the results are here embodied in a form highly creditable to our medical literature. He is very safe in indulging the hope, that his work "may be thought by those whose approbation he would be most ambitious to obtain, to contribute somewhat to the study of the natural history of the several forms of continued fever, and to the aggregation of facts having important bear-

ings on diagnosis, pathology and therapeutics." His statistical details possess uncommon interest, and in his use of them he shows a mind at once cautious, logical and comprehensive. The reader would search in vain through the volume for one single instance of hasty deduction. All is fair, unprejudiced, calm, and philosophical. The author had no cherished theory to sustain by bending facts to suit it, and when it happened that he held an opinion which his inquiries did not sanction he unhesitatingly abandoned it. Thus, he set out a believer in the identity of the fevers in question, but before he completed his reports he announces that "the study and thought bestowed upon the subject" had led to a change in his views. As a specimen of cautious induction, the work is worthy of study, especially in our profession, which has abounded more than most others in examples of loose, inconclusive reasoning. The style in which it is composed is also admirable for a scientific work. It is quite evident that Dr. Flint has not regarded style as a matter to be entirely overlooked even in such a work, and that he has cultivated it with great success. One is not only satisfied as he proceeds with the fairness of his argument, and instructed by his full, lucid details, but pleased and gratified by the easy, natural, graceful flow of his language.

In connection with these reports, Dr. Flint treats of the identity of Typhus and Typhoid fever; the diagnosis differential of the two affections; the discrimination of continued fevers from other diseases; the management of continued fever; the morbid appearances after death; relapsing fever; and the transportation and diffusion, by contagion, of Typhoid fever, exemplified in its occurrence at North Boston, Erie county, New York. As the language of the last member of this sentence implies, he believes in the contagiousness of the disease. He thus forcibly puts the case:—

"In a small, isolated community, consisting of nine families, seven of which lived within a few rods of each other, a person is introduced affected with *Typhoid* fever, and after lingering twenty-nine days, dies with this disease. Prior to that event the members of this community were free from disease of any kind—the situation was in every respect healthy, and *Typhoid* fever was a disease unknown in that place and neighborhood. The patient lingered and died at the public house, which was a place of daily resort for the members of those seven families with a single exception. One family, consisting of several persons, living but four rods from the tavern, was on terms of hostility with the inn-keeper which precluded all intercourse. The arrival of a sick stranger with a severe disease was an event of interest to the inhabitants. He was visited, more or less, daily by the different members of the families living

closely at hand, with the exception of the family referred to; and the family of the inn-keeper were of course brought into close contact with the disease. Twenty three days after the arrival of the stranger, two members of the family of the inn-keeper were attacked with the disease, and subsequently five others in his family. In all the other families living within a few rods of the tavern, excepting a single family, cases occurred, more or less in number, within the space of about a month from the date of the first case developed after the stranger arrived; and during this period more than half the population of the settlement were affected. The disease then ceased further progress, no cases afterward occurring. The family in which no cases occurred was the only family of the seven not brought into contact with the disease. The relations of this family to that of the inn-keeper, as just stated, precluded all social intercourse, and shortly after the disease began to spread, its production being imputed to the agency of this family, all intercourse with the other families affected with the disease was at once suspended.

"Now in view of this reviewal of the facts, if it be claimed that the disease was not transported to the place and diffused by contagion, it is necessary to admit a series of coincidences almost incredible. It will be seen that the circumstances which have just been rehearsed, embrace all those which had been previously mentioned as important conditions for a fair experiment in order to test the contagiousness of a disease. In fact, if every circumstance connected with the history of the disease at North Boston had been deliberately selected and arranged for a scientific object, they could hardly have been rendered more complete or judicious."

Dr. Flint closes his remarks on the management of continued fever, with a sentiment so just and important, that we are glad to have an opportunity of reproducing it for the benefit of our readers:—

"There is one point, pertaining to diet, respecting which the views commonly entertained appear to me to be faulty. I refer to the apprehensions frequently, if not generally felt by practitioners, in allowing a nutritious animal diet early in convalescence. These apprehensions, which I am persuaded are imaginary, have probably arisen, from the habit of associating Inflammation with Fever. The susceptibility to disorder, or consecutive diseases, is probably owing to the enfeebled condition of the organism after passing through the febrile career. It is an object, therefore, in the management of convalescence, to effect a return of the accustomed constitutional vigor as speedily as practicable. The means for this end are, aliment of the most digestible and nutritious kind, tonics, stimulants, and hygienic measures, of which one of the most useful is early gestation in the open air. I have pursued this course in a sufficient number of cases to feel satisfied from experience that it is not only unattended by danger, but is conducive to the well being of the patient. I have never witnessed any unpleasant results from the effort to re establish the strength of the patient by resorting at once to an animal diet, etc."

We received this work too late for a full notice of its contents in our present number. From what we have said, our readers will gather that our opinion of it is a very favorable one. We believe that it will take a high rank among the professional works of our country, and a permanent place in the literature of continued fever. It is a model for such researches, and in this light will be most highly appreciated by those who have had most experience and are most skilled in the investigation of disease. It is very carefully printed, and forms a neat octavo volume of 398 pages. The publishers are H. Derby & Co., Buffalo. We are indebted for a copy of it to T. R. Nelson, of this city.

KENTUCKY MEDICAL SOCIETY.

We are glad to see from the subjoined circular, that the *Committee of Arrangements* have directed public attention to the meeting of the State Medical Society soon to take place in this city, and we would cordially unite with the committee in the expression of a hope, that the meeting will be numerously attended. We have reason to know that great labor has been bestowed by some of the committees appointed to report on professional subjects at the approaching meeting, and cannot doubt that the meeting will be one of unusual interest.

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The committee of Arrangements beg leave to call the attention of the physicians of the State to the second annual meeting of the Kentucky Medical Society, to be held in the city of Louisville in October. The meeting, according to adjournment, will take place at 1 o'clock, P. M., on the 20th of that month, in the Court House. The committee would inform their brethren throughout the State, that ample provision has been made for the reception and entertainment of the members, and hope that the meeting will be a large one, as it promises to be a highly interesting one. Various reports may be expected from committees appointed at the last meeting, and from the ability of the gentlemen engaged upon them, it is safe to promise that they will be full and instructive.

N. B. ANDERSON, M. D., *Chairman.*

THE

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OF

MEDICINE AND SURGERY.

OCTOBER, 1852.

ART. I.—*Observations on Excision of the Inferior Maxillary Bone; Illustrated by Five Cases.* By S. D. GROSS, M. D., Professor of Surgery in the Medical Department of the University of Louisville.

In the last number of this Journal, I published a paper on excision of the upper jaw, preceded by a brief account of the more frequent and important diseases requiring this operation, and followed by a history of seven cases in which this bone was removed, either wholly, or in part. It is my intention, in the present communication, to offer some remarks upon exsection of the lower jaw, accompanied by several illustrative cases.

The maladies of the two jaws are so much alike, in their essential features, that the remarks which were made, in the preceding number of this Journal, respecting those of the one, may be received as strictly applicable to those of the other. Indeed, if there is any difference in this

particular, it must be ascribed wholly to the modifying influence of the antrum of Highmore; though it would be difficult to determine, in the present state of pathology, in what this influence consists, supposing it to be real and operative. All that we know, with any positive certainty, is, that medullary disease is much more frequent in the upper than the lower jaw, and the only means which we have of accounting for this circumstance, is the existence of the maxillary sinus, which, being lined by a mucous membrane, and charged with the execution of an important office, is peculiarly prone to take on this kind of action. The inferior jaw, on the contrary, is a solid structure, destitute of mucous tissue, and is, as all experience proves, particularly subject to fibro-cartilaginous, epuliform, and cystic formations.

My experience is that *encephaloid* of the inferior jaw is much more frequent in children and young adults than at any other period of life. Indeed, the very worst cases that I have seen of this disease occurred before the tenth year, and ran their course with a rapidity truly frightful. Most of the sufferers thus affected die within six or eight months from the commencement of the attack; and if an attempt be made to relieve them by operation, the malady is almost sure to return in a very short time, either at the cicatrice, or in the adjacent structures, especially the lymphatic ganglions. The prognosis here, is, if posible, still more unfavorable than in medullary disease of the superior jaw.

The *epuliform* tumor is much more common in the lower jaw than in the upper; indeed, it appears to have a special predilection for this bone, springing up, generally, without any obvious cause, or under the influence of the most trifling irritation, and often acquiring a very extraordinary magnitude in a few weeks. In all the cases of this disease that have fallen under my observation, excision of the tumor, unless the operation embraced the bone at the site

of the excrescence, was worse than useless. The malady has nearly always a most astonishing repullulating tendency, though the action which accompanies it is not always, perhaps, indeed, not generally, malignant in its character. Still, the proper rule of practice, in such a case, is to sacrifice a portion of the affected bone, taking care, as a means of safety, to cut it away some distance beyond the limits of the morbid growth. My fifth case affords a remarkable example of the reproductive, persecuting tendency, if I may so express myself, of this singular disease.

The *fibro-cartilaginous* tumor of the lower jaw is capable of acquiring an immense magnitude. Instances are recorded where it was as big as the patient's head, accompanied with the most hideous and disgusting distortion of the features. In general, it is slow in its growth, and unattended with contamination of the system and of the adjacent parts; circumstances in its history of the greatest diagnostic value: when removed it manifests no disposition to return. Examined with reference to its structure, it is found to contain, usually, a considerable number of cells, filled with various kinds of substances, serous, glairy, sanguineous, and purulent, surrounded and traversed by osseous spicula, and fibrous, fibro-cartilaginous, and cartilaginous septa. The compact structure is commonly absorbed, or softened and broken up; and, occasionally, the greater portion of the bone is converted into a hollow shell, separated into different compartments, and occupied by different fluids.

There is a peculiar tumor of the lower jaw, to which I have ventured, in my "Elements of Pathological Anatomy," to apply the term *hæmatoid*, as most expressive of its true character. It consists essentially in an expansion of the two lamellæ of the bone into a cavity, or hollow, occupied by dark coagulated blood, tolerably firm in its consistence, and evidently in a state of partial

organization. The walls of the tumor are thin, but firm, sometimes elastic, at other times inelastic, and composed entirely of the compact tissue, invested by its periosteum. Its volume varies from that of a pigeon's egg to that of an orange. It is of slow formation, and is perfectly free from malignancy. A good specimen of this tumor is afforded in the case of Smith, the first on the list. I am not aware that this disease has ever been met with in the upper jaw.

The lower jaw is sometimes expanded, at one or more points, into a hard, firm, solid tumor, constituting a species of *hypertropky*. The density of the affected part is occasionally equal to that of ivory. A few years ago, Dr. Pinkney, a highly intelligent surgeon of the United States Navy, showed me a piece of the body of the inferior maxillary bone, which he had removed for an affection of this kind from a man at Lima, and which was so hard that he found it almost impossible to divide it with the saw.

Excision of the lower jaw is sometimes required on account of *necrosis*. About five years ago I was consulted by a young man of the name of Bainbridge, from Cincinnati, respecting an affection of this kind, induced by the action of mercury, administered profusely, some months before, during a severe attack of remittent fever. He had suffered very much, and his mouth, gums, and jaw were in a horrible condition. Upon examination, I found a portion of the bone on the right side perfectly denuded, and as black as a coal; it was quite movable, and was easily extracted with a pair of bone-forceps, without any incision, except a slight internal one. It proved to be a part of the body of the jaw, with the ascending ramus, and the coronoid and condyloid processes. A case recently occurred in which the entire lower jaw was removed for an affection of this description, the surgeon making an incision from ear to ear in

order to accomplish his object. It seems to me that in such a case the bone might be easily removed without any external incision whatever, simply by dividing it at the chin, and then extracting each piece separately with the forceps. Exsection, properly so termed, can hardly be required under such circumstances. If the parts be loose, as they generally are, or ought to be, before an attempt is made at exsection, a single cut will often suffice to liberate them. Where the necrosis is very extensive, or the exfoliation imperfect, a more free dissection will, of course, be necessary. The eclat of an operation should never induce a surgeon to inflict improper mutilation upon his patient. It is bad enough, in all conscience, for a man to lose his jaw, but to be, at the same time, unnecessarily disfigured, is unpardonable. The generous soldier never cuts off the ears, or pulls out the eyes of his enemy after he has vanquished him. He contents himself with the execution of an imperative and indispensable duty.

Portions of the inferior maxillary bone have been removed on account of *caries* of its substance, and even for malformation merely of the coronoid process, impeding the evolution of the wisdom tooth. The cases requiring such an operation, for such a cause, must be very rare indeed.

In concluding these brief remarks upon the nature of the principal affections which require amputation of the lower jaw, I wish to say a few words respecting the changes which the *coronoid and condyloid processes* are so apt to undergo in some of the abnormal growths of this bone. It is a singular fact, and one which does not appear to be generally known, that these processes are seldom the seat of any new deposit, whether benign or malignant. They seem to possess a remarkable conservative power. Nevertheless, there are few cases in which the bone is extensively involved, where they do not experience impor-

tant alterations, not so much from any extension of the disease, as from the effects, apparently, of ordinary inflammation, developed under the influence of the irritation caused by the encroaching tumor. The most common changes which they undergo, so far as my observation enables me to judge, consist in their conversion into thick, irregular knobs, more or less porous and unhealthy; sometimes preternaturally firm, at other times quite soft and disorganized. The periosteum of the coronoid process and neck of the bone is generally, in such cases, considerably thickened, and the ligamentous connection of the temporo-maxillary articulation so much altered as to be scarcely distinguishable; the inter-articular cartilage is absorbed, or so adherent to the contiguous surfaces as to cause partial, if not complete anchylosis. Occasionally the coronoid and condyloid processes are very much wasted, or even entirely annihilated. Owing to these various changes, it is usually easy to effect the liberation of these processes, while the reverse is the case when they retain their normal character.

It is not necessary, at this late day, seriously to discuss the *claims* of any one to the credit of priority in this operation. This credit, and it is a credit of which any one may justly be proud, is now universally conceded, by all enlightened surgeons, both at home and abroad, to a Western physician, Dr. W. H. Deaderick, formerly of Rogersville, now of Athens, Tennessee. How this gentleman, who must have been very young at the time, came to undertake an operation which was not only formidable in itself, but without a precedent in the annals of surgery, is entirely a matter of conjecture. It may be supposed, however, that he was an excellent anatomist, and a bold, adventurous operator, who did not hesitate to think and to act for himself. Dr. Deaderick is still living. and long may he continue to enjoy the fruits of an honorable and well-spent professional life.

The case of Dr. Deaderick, requiring the operation under consideration, is too interesting and important to be passed in silence. The patient was a lad, fourteen years of age, who had a tumor of a cartilaginous structure, on the left side of the jaw, filling nearly the whole of the mouth, and causing great difficulty in swallowing, and even, occasionally, in breathing. The operation was performed on the 6th of February, 1810.* An incision was commenced under the zygomatic process, and carried across the tumor, in the direction of the lower jaw, to nearly an inch beyond the middle of the chin. From the centre of this, and, consequently, at a right angle with it, another incision was extended a short distance down the neck. The flaps thus marked off being separated from the diseased mass, the bone was sawed off immediately at the angle and centre of the chin. The wound was united in the usual manner, and the boy had a speedy and happy recovery; continuing perfectly well thirteen years after the operation.

The next operation of this kind upon the lower jaw was performed by Dupuytren, on the 30th November, 1812, without, it may be presumed, any knowledge, on his part, that he he had been anticipated in this undertaking by any one in America; for it is a singular fact that no account of Dr. Deaderick's case appeared in print until thirteen years after its occurrence, so regardless was this gentleman of any reputation that might be supposed to arise from its publication. In November, 1821, Dr. Mott, of New York, excised nearly the whole of the inferior jaw on one side; and, eighteen months afterwards, he removed all that portion of the bone which intervenes between the right temporo-maxillary joint and the second bicuspid tooth on the left side. This, so far as I can ascertain, is the first instance in which exarticulation was

* American Medical Recorder, vol. 6, p. 516.

attempted in the United States. Two years prior to this, Dr. Graefe, of Berlin, performed a similar operation; and in 1816, Mr. Anthony White, of London, removed half a necrosed jaw from its socket.

From all that precedes, it is evident that Dr. Deaderick is entitled to the credit of having been the first surgeon that ever excised a *portion* of the lower jaw; and, secondly, that Dr. Mott was the first, in this country, who romoved the bone at the temporo-maxillary articulation.

In my article on excision of the upper jaw, in the last number of this Journal, I had occasion to condemn, in a very pointed manner, the *ligation* of the primitive carotid artery, as a means of avoiding hemorrhage, in amputation of the maxillary bones. I recur to the subject here, because I find it stated, as late as 1846, in a work which is extensively read by the profession in the United States, that Dr. Mott lays it down as a principle, in all severe cases requiring extensive exsection of the lower jaw, to take up the primitive carotid artery as a preliminary and indispensable step, in order to cut off the dangerous hemorrhage which would otherwise ensue from the division of its main branches.* I do not wish to criticise the practice of Dr. Mott in a department of operative surgery, with the advancement of which his own name and fame are so intimately and so honorably associated; but it does seem to me that it would have been well had he expressed himself with more caution on so important a subject. Whatever Dr. Mott may utter is received by certain physicians, especially by his own pupils, as oracular, and it is for this reason that I am induced to notice, and I do it with all possible candor and good feeling, the practice here referred to. I humbly submit whether it would not have been better if this eminent surgeon had confined him-

* Velpeau's Operative Surgery, by Townsend & Mott, vol. 2, p. 909: New York, 1846.

self to saying that *he* had found it necessary, in his more severe and formidable cases, to tie the carotid artery as a preliminary measure, than to assert that it was an "indispensable step," and that, therefore, it should always be adopted, under such circumstances, as a *principle?* The ligation of this vessel is, in general, easy enough; but the consequences are what we are to look to, and these are not to be regarded too lightly. In one of Dr. Mott's cases, where the carotid artery was secured immediately before the disarticulation of one-half of the jaw, the hemorrhage was extremely profuse, and not less than from fifteen to twenty vessels required to be tied during and after the operation. In another instance, notwithstanding the greatest precaution was employed, the patient became so faint and exhausted, and the mind so much perturbed, during this "preliminary and indispensable" operation, that the excision of the tumor was obliged to be deferred until the following day. Dr. Warren, of Boston, in a similar undertaking, wounded a vein in searching for the carotid, and came very near losing his patient from the entrance of air into the circulation. The jaw was removed a week afterwards without ligating the artery. Mr. Lizars, of Edinburgh, in attempting to excise the superior maxillary bone for a medullary tumor of the antrum, commenced by tying the carotid on the affected side, but was prevented from proceeding by excessive hemorrhage, the patient having lost upwards of two pounds of blood in a few seconds.*

But the above are not the only objections to the performance of this operation in cases of excision of the jaw. Ligation of the primitive carotid artery is often followed by serious lesion of the brain, and by severe and even fatal inflammation of the air-passages. The inter-

*See Dr. Norris's interesting Statistics, Amer. Journal Med. Sciences, vol. 14, p. 31.

estingfacts and tables of Miller and Norris upon this subject are conlusive; and they should serve to admonish us never to resort to this operation, except under circumstances of the most positive and pressing character. To complicate, without any necessity, a case that is in itself already sufficiently severe and hazardous, is to augment the chances of a fatal termination, and to sport wantonly with human life. It ought to be sufficient glory for an operator, if glory he must have, to limit himself to the performance of what is strictly needful. To go beyond this, in any particular case, is to bring discredit both upon himself and upon the science of surgery.

In the only three instances in which I have removed the jaw at the temporo-maxillary articulation, I did not find it necessary, in two, to use a single ligature, and in the other, involving an immense tumor, not more than two or three small vessels were tied. These examples, so far as they go, confirm the opinion of Liston, Syme, and others, upon the propriety of omitting the ligation of the primitive corotid, as a preliminary measure in exsection of this bone.

It would be a waste of time to attempt here to lay down rules as to the best mode of *performing excision* of the lower jaw. The works on operative surgery now before the profession, are so numerous and so able as to preclude the necessity of any labor of this kind. There are a few points, however, upon which I deem it proper to comment, inasmuch as they are not generally dwelt upon by writers with sufficient emphasis; if, indeed, some of them have not been wholly overlooked.

One of the most important circumstances to be observed in exsection of the lower jaw, according to my experience, is to keep in close contact with the morbid structure, and yet sufficiently away from it to prevent any portion of it from being left behind. This would seem almost like a contradiction, and yet it is, in reality, anything but

a contradiction. By attention to this rule, which I regard as of paramount importance, two great ends are attained: the easy removal of the tumor by a neat and rapid dissection, and the avoidance of hemorrhage. Cutting into the tumor is almost certain to be followed by the division of large vessels, which do not fail to bleed profusely, unless checked by compression or other means, until the operation is completed. Besides, chipping off a piece here and a piece there leads to a more or less tedious after-dissection, which is always attended with pain to the patient, and annoyance to the operator.

Another important rule is, to work, as much as possible, with the handle instead of the edge and point of the knife, especially when we are detaching the bone from the soft parts. Whenever it can be done, a portion of the periosteum should be saved, and there are few cases in which this membrane is so thoroughly involved in the disease as to render this impracticable. The part thus rescued is of great importance afterwards in filling up the void produced by the removal of the bone, at the same time that it prevents undue injury to the other soft structures.

The *external incisions* should always be made in such a manner as to avoid the unsightly appearance resulting from a large and exposed scar. For this purpose, when it is designed, for example, to remove the half of the jaw at the articulation, the knife should, as a general rule, be carried along the base of the bone, from the zygomatic process, about three-quarters of an inch in front of the ear, to the chin, and thence some distance up the midian line, or even as high up as the red margin of the lip. When the tumor is of immense size, two incisions are sometimes required, so as to include an elliptical portion of the soft parts; but, unless this is the case, or the skin is seriously involved in the disease, not a particle of integument should be sacrificed; for, during the healing process there is usually inordinate contraction, and hence

if this circumstance is not attended to, great and disagreeable deformity will be the inevitable consequence. By making the perpendicular incision some distance in front of the ear, there will be little danger of wounding the temporal or external carotid artery, and the trunk of the portio dura. Sometimes, as when the disarticulation is effected with difficulty, a short horizontal incision, just below the zygomatic process, will greatly expedite our efforts; but, in general, this is unnecessary. The duct of Steno should always, if possible, be avoided, as it may usually be by being careful not to carry the knife too high up, or too far forward.

When the alveolar process alone is involved, the affected part may often be removed without any external incision at all, simply by detaching the lip or cheek from the bone, and holding it out of the way, while the osseous structures are divided with the saw. Conducted in this manner, the operation is productive of hardly any disfigurement.

When the operation involves the removal of the jaw at the joint, the best plan is to expose the tumor as rapidly and carefully as possible, and then saw the bone at the anterior limits of the morbid mass. This greatly expedites not only the process of disarticulation, but the separation of the jaw from its muscular and mucous attachments, as it enables the operator, by taking hold of its anterior extremity, to move the bone in any direction he pleases.

When the alveolar process alone is affected, or along with only a portion of the body of the jaw, it is a good rule, as was stated, long ago, by Dr. John Rhea Barton, of Philadelphia, to save the base of the bone, nay, even the merest strip of it, in order that it may serve as a support to the soft tissues, and become the nucleus of a new deposit.

One of the difficulties connected with this operation relates to the liberation of the coronoid and condyloid

processes. The instrument which has always been employed for this purpose is the knife, or the knife and saw. The fibres of the temporal muscle, embracing the coronoid process on every side, are directed to be cut close to their attachments, or, instead of this, the process is sawn through at its base; the structures of the temporo-maxillary articulation are always divided with the point of the knife, entered at the anterior or posterior part of the joint, as may be most convenient. Now, it has always appeared to me that this mode of procedure should, if possible, be avoided, as it is apt to be followed by serious hemorrhage, and by injury of important nerves. This is especially the case with regard to the separation of the condyle, lying as it does in close and intimate relation with the internal maxillary artery, which must necessarily be endangered by the knife in this stage of the operation. A wound of this vessel, just as the operation is about to be finished, is an embarrassing circumstance, from the difficulty of applying a ligature, and is liable to be accompanied with copious hemorrhage. The coronoid process, although it projects up some distance into the zygomatic fossa, is separated with less difficulty, and, as it lies anterior to the maxillary artery, there is but little danger of interfering with this vessel. Still, a pretty smart hemorrhage occasionally results from the division, simply, of the little arteries of the temporal muscle.

To obviate this danger, as well as to expedite the process of disarticulation, usually, and, indeed, very justly regarded, in the ordinary mode, as no very easy part of the operation, I have used, with great advantage, an instrument combining the principles of a lever and a knife. The accompanying sketch will convey a much better idea of it than the most elaborate description. The blade is slightly curved upon the flat, and is three inches and a quarter in length, by three-eighths of an inch in width; its thickness is about one line and a third. Its free ex-

tremity terminates in a convex edge, beveled off in front and behind, so as to admit of being used for dividing the periosteum, or scraping the bone, as may be deemed necessary. The other extremity is set in a stout rough handle, nearly four inches long. A perfectly straight instrument of this kind may be used with much advantage.

In the only three instances in which I have had occasion to remove the jaw at the temporo-maxillary articulation, I have found, in two, both the condyloid and coronoid processes a good deal enlarged, apparently from simple hypertrophy, and the periosteum very considerably thickened; thereby allowing itself to be easily pealed off from the bone. Whenever this is the case, and my belief is that the occurrence is very frequent, the liberation of the parts in question may be readily effected with the instrument which is here delineated. All that is necessary is to insinuate the convex, or semi-blunt edge beneath the fibrous covering of the coronoid process, and after separating it for some distance, to prize the bone out of its place. In the same manner the soft structure may be detached from the condyle of the jaw, and the latter raised out of the glenoid cavity. The whole is the work of a few seconds, and the great beauty of the proceeding is, as before stated, its entire freedom from danger to the maxillary and other arteries, as well as the trunk and deep-seated branches of the portio dura. Where these processes with their investing structures are

perfectly sound, the separation must be effected, at least in part, with the knife, but even here the instrument above described may afford valuable aid.

I have, within the last few days, had an opportunity of testing this instrument in a case of disarticulation of the jaw, unattended with the slightest change of structure in the condyloid and coronoid processes. The operation was unusually difficult, on account of the shortness of the affected bone, portions of which had been previously removed on two different occasions. After having dissected off the skin, the bone was thoroughly denuded in every direction, both of periosteum and muscles, with the instrument in question. The parotid gland was but little injured, and the duct of Steno and the trunk of the portio dura were, there was reason to believe, entirely intact. No important artery was wounded; a circumstance which could hardly have been avoided had the separation of the coronoid and condyloid processes been effected with the knife. The bone, at the close of the operation, was completely devoid of periosteum, and even of the cartilaginous incrustation of the condyloid process.

The *gap* left by this operation is occasionally, perhaps frequently, filled up, especially in young subjects, by a cartilaginous formation, of an irregularly cylindrical shape, which, while it serves to support the jaw in mastication, assists materially in reestablishing the symmetry of the features. The time required for the production of this substitute varies, it may be supposed, in different cases, from a few months to several years. Even when one-half of the bone has been removed, nature sometimes succeeds most admirably in her efforts. In 1832, I had an opportunity of seeing an Irish lad, aged about seventeen, from whom the late distinguished Dr. Cusack, of Dublin, had removed, four years previously, the left half of the inferior maxilla, on account of a fibro-cartilaginous affection. In this instance, nature had made an attempt

at reproduction, the part being replaced by a thick, rounded piece of cartilage, sufficiently strong to subserve the ordinary purposes of mastication. It is astonishing what little deformity of the features there was in this case.

CASES OF EXCISON OF THE LOWER JAW.

Case 1.—David L. Smith, thirty-five years of age, a farmer, of thin, spare habit, from Ohio county, Kentucky, visited Louisville early in October, 1844, on account of an affection of his lower jaw. The disease was first noticed, about three years ago, in the form of a hard, solid tubercle, not larger than a hazel-nut, on the left side, just behind the cuspid tooth. Its progress was very slow until last Christmas, when it began to increase with considerable rapidity; and it soon became the seat of a constant pain, of a dull, aching character. The malady made its appearance without any assignable cause. The patient had never sustained any injury of the part affected; and his general health had always been good, with the exception of an occasional attack of dyspepsia.

When Smith placed himself under my care, the tumor was exceedingly hard and firm, and bulged forward in such a manner as to cause considerable deformity of the left side of the face. It extended from the lateral incisor on the right to the middle of the large grinder on the left. The corresponding teeth inclined backwards and inwards, and were so loose as to be unfit for the purposes of mastication. The gum was of an unnaturally red color, and somewhat hypertrophied, but otherwise perfectly healthy. The mucous membrane and cheek were perfectly sound, and there was no enlargement of any of the surrounding lymphatic ganglions.

Excision of the tumor was performed on the 10th of October, in the presence of Professor Miller and Drs. Johnston, Cochran, T. L. Caldwell, Colescott, Bayless, and Somerby, the distinguished dentist. The patient sat upon a chair with his head supported upon the breast of

an assistant; and, as a preliminary step, Dr. Somerby extracted the first large grinder on the left side and the right lateral incisor. A rectangular incision was then made, commencing at the free margin of the lower lip, a little to the right of the median line, and extending down to the base of the chin, and from thence backwards, along the outer and inferior surface of the jaw, as far as the posterior limits of the disease. The flap thus formed was carefully dissected off from the tumor, and held out of the way, while the bone was sawed through, first, on a level with the right canine tooth, and secondly just in front of the middle grinder on the left side. The coronary artery was controlled by the fingers of an assistant. In consequence of the excessive hardness of the bone, great difficulty was experienced in dividing it; this, however, was at length accomplished with a saw constructed for the purpose, and the tumor removed. In pulling the tumor downwards, a portion of its posterior surface, a mere shell, broke off, and, as it was perfectly sound, was left behind.

The edges of the wound were brought together by four twisted sutures, the first of which was introduced near the red margin of the lip. No vessels required to be tied, and scarcely three ounces of blood were lost. A piece of lint was placed within the mouth to support the chin, and opposite to this, on the outside, a compress; the whole being confined by a roller carried round the head and jaw.

For several days after the operation, Smith had some difficulty in swallowing, and a considerable swelling of the lip, with a copious discharge of saliva, and some febrile commotion. These symptoms speedily yielded to an active cathartic and cooling drinks. At the end of the fourth day, the three lower needles were removed, and on the fifth the upper; the union was complete. Suppuration ensued in forty-eight hours in the track of the bone,

granulations soon began to form, and in less than three weeks the mouth was perfectly healed. The cicatrice is scarcely perceptible; and, with the exception of a slight retrocession of the chin, there is no deformity.

The tumor, about the volume of a medium-sized orange, was found to consist of a mere osseous shell, without any vestige of the cancellated structure, and was occupied by three red, solid coagula, the largest of which did not exceed the volume of a pigeon's egg. The cavity was only partially filled by the clotted blood, which adhered to the inner surface of the bony wall, and was evidently organized. I have repeatedly heard from Smith since the operation, and am happy to say that there has been no return of his disease, which was certainly not malignant.

**Case* 2.—Kitty, a negress, nine years of age, was brought to me for professional advice, on the 2d of May, 1846, by her master, Mr. Lawton, of Rumsey, Kentucky. Eighteen months previously she punctured the left side of her face with a piece of wood, which she happened to have in her mouth as she fell, head-foremost, from a high fence. Some inflammation ensued, but this rapidly disappeared, and nothing further was thought of the accident until four months after, when a slight enlargement was noticed at the original site of the injury. This gradually extended until it involved the whole of the left side of the jaw, from the ear to within a few lines of the symphysis of the chin, and from the middle of the cheek to the upper part of the neck. The tumor, which had increased very much within the last three or four weeks, was very prominent externally, and reached, at the time of my examination, when the head was held in its ordinary position, nearly down to the clavicle. Articulation and mastication were both very much impeded, and the jaw was so completely anchylosed that it was almost impossible to move it in any direction; indeed, the interval

* American Journal Medical Sciences, vol. 16, p. 344.

between the dental arches was so small as not to permit the protrusion of the tip of tongue. The swelling was very hard externally, and towards the base of the jaw were two small points, which seemed to have been the seats of little abscesses, probably of a scrofulous character. In other respects the skin was perfectly sound. Internally, the bone appeared to be healthy, and did not encroach upon the tongue. The tumor was at first free from suffering, but during the last month, the girl complained of a good deal of pain, especially at night. The general health was unimpaired.

Such being the condition of the parts, it was obvious that nothing short of excision of a portion of the jaw could afford any relief. The operation was performed on the 5th of May, in the presence of Dr. Colescott and Dr. Thomas L. Caldwell, and a number of medical students. As a preliminary step, the body and limbs of the little patient were surrounded by a strong apron, carefully pinned behind, as in the operation for harelip. She was seated upon a chair, with her head inclined a little backwards, and supported by an assistant. An incision was made through the middle of the lower lip to the base of the chin, and, at a right angle with this, another along the body of the diseased bone. The flap thus marked out was then dissected up, and the jaw sawed through a little obliquely, immediately by the side of the symphysis, so as to preserve the two incisors. An incision was then carried from the tragus of the ear to the termination of the horizontal one, already mentioned; the integuments were rapidly dissected up; the masseter, temporal, mylohyoid, and pterygoid muscles were successively separated from their attachments; and the bone was finally raised from its socket. Not a little difficulty was experienced in this stage of the operation, from the cartilaginous adhesions between the coronoid and zygomatic processes, the former of which was very much thickened, and forced the latter out in such a manner as to render the cheek

quite prominent, in that situation. The temporo-maxillary joint was completely anchylosed, and the attachments formed by the condyle rendered the disarticulation almost impracticable.

A part of the sub-maxillary gland appeared to be changed in structure, and was accordingly removed. Several lymphatic ganglions of the neck were, for the same reason, excised. The operation was exceedingly painful; and, as I was about to disarticulate the bone, the patient swooned away. Scarcely six ounces of blood were lost, and, what is remarkable, not a single vessel required to be tied. The bleeding from the facial artery, which was divided in the early stage of the operation, was readily controlled by the finger of an assistant.

The tongue, stylo-hyoid muscle, and submaxillary gland were fully exposed by the dissection; the parotid lay at the posterior part of the wound, and the external carotid was seen pulsating just below the ear.

The operation being over, the wound was kept covered for nearly three hours with a thick linen compress, frequently wet with cold water. At the expiration of this period, the edges were brought together by six twisted sutures, and a few adhesive straps, the whole being supported by a thick compress and a roller.

Very little pain was experienced after the operation, and the patient slept comfortably during the succeeding night. On the following day there was slight fever, with some swelling on the left side of the face, but this yielded speedily to a dose of Epsom salts and cooling drinks. The needles were removed at the end of the third day, and the parts were found to be beautifully united, both externally and internally. Subsequently the temple and cheek were invaded by erysipelas, brought on, apparently, by premature exposure and improper eating. A dose of calomel, and the application of tincture of iodine, promptly put a stop to this disease; and from this time on, the convalescence was rapid and uninterrupted.

The tumor, on dissection, was found to be of a fibrous character, dense, inelastic, and of a dull whitish color. The periosteum, from which it probably arose, was considerably thickened, and could be peeled off, without difficulty, from the subjacent bone. The bone itself was considerably enlarged, and a section of it exhibited a uniform, compact surface, with here and there a small elongated cell. The coronoid and condyloid processes were remarkably thick, and the latter had lost its smooth and polished appearance. The body of the bone contained a perfectly healthy molar tooth.

Upwards of six years have now elapsed since the above operation, and there is, thus far, no evidence whatever of any tendency to a relapse of the disease for which it was undertaken. Kitty's master has recently informed me that she is entirely well, that there is hardly any deformity of the face, and that she chews, articulates, and swallows with as much facility as any one.

Case 3.—Milton, a colored man, aged 38, the father of four children, and a servant of Henry C. Ewin, Esq., of Todd county, Kentucky, was sent to me in April, 1850, on account of an enormous tumor of the left side of the lower jaw, which had latterly become a source of great suffering and annoyance to him, so much so, indeed, as to disqualify him for active exertion, and deprive him, almost wholly, of the power of mastication. The morbid growth had existed about twelve years, during the last four of which it had increased very rapidly, giving rise to the most hideous and disgusting deformity, and rendering the poor fellow an object of commiseration with every one who beheld him.

The dimensions and limits of the tumor, upon the arrival of the patient in Louisville, were as follows: From the angle of the mouth, in a straight line to the ear, nine inches; from the chin to its posterior extremity, eleven inches and three-quarters; and in the vertical diameter,

ten inches and a half. Terminating an inch and a half behind the left ear, it extended above, on a level with the zygomatic process and ball of the eye, and below to within about two inches of the clavicle, when the head was forcibly bent back, while it almost touched that bone when the head was in its natural position. It overlapped the upper part of the thyroid cartilage, and dragged the mouth so much over to the right side, that the left angle of this outlet was habitually on a line with the septum of the nose. The outer surface of the tumor was quite uniform, except towards its base, where two small openings existed, giving vent to a thin, viscid, slightly purulent fluid, and environed each by a well-marked cicatrix; here and there was a softish and elastic spot, and just in front of the ear it imparted a sensation as if it contained a fluid.

Viewed internally, the morbid growth was found to extend beyond the median line, throwing the tongue, which reposed, as it were, upon its upper surface, completely upwards against the roof of the mouth, and rendering it almost impossible to protrude it beyond the lips. Posteriorly the tumor reached tothe arch of the palate, greatly diminishing the opening of the fauces, and seriously embarrassing deglutition, articulation, and even respiration. Its inner surface was smooth, shining, and elastic, and was marked by a groove, caused by the grinders of the superior jaw.

When the tumor was first noticed, and for a long time afterwards, it was free from pain and even uneasiness; during the last two years, however, it was the seat of a constant aching feeling, and not unfrequently of a keen, darting pain. The general health had been good; but latterly the man had lost strength and flesh, and was unable to sleep well at night. No cause could be assigned for the origin of the tumor.

Such was the frightful condition of my patient after he had been for some time under my care, that I was greatly

in doubt whether an attempt should be made to rid him of his burden. An eminent professional gentleman who saw him with me, about ten days after his arrival in Louisville, in fact, strenuously opposed the operation, on the ground that it would almost certainly be followed with fatal results, if not at the time, at all events very soon after, and that it would thus tend to bring surgery into discredit. The impression which the interview made was not without some weight; but my feelings for the poor fellow at length overcame my scruples, and I determined to afford him a chance for his life. He had undergone ample preparatory treatment, and was therefore in excellent condition for the knife.

The operation was performed on the 13th of May, at the amphitheatre of the Louisville Marine Hospital, in the presence of my friend Dr. Lopez, of Mobile, Alabama, and a number of physians and students. Professor Miller, Dr. Goddard, the dentist, Dr. T. G. Richardson, and my pupils, Dr. Thomson and Dr. Trabue, kindly afforded me their assistance. The patient, placed under the influence of chloroform, lay upon the table, with his head resting upon a pillow, and his face inclining towards the right side. The left central incisor having been extracted by Dr. Goddard, I made an incision over the prominent portion of the tumor, commencing half an inch in front of the ear, and just beneath the zygomatic process, and extending forwards in a semi-circular direction as far as the centre of the chin, and thence upwards along the middle line through the free margin of the lip. Another incision, beginning near the posterior extremity of the first, was then carried forwards for some distance over the central portion of the swelling, so as to include the unsound skin in the form of an ellipsis. Several vessels, among others the external maxillary artery, were divided, and immediately compressed by the fingers of my assistants. Detaching the lip and integuments of the chin, I at once divided the

jaw at the symphysis with a metacarpal saw; a procedure which occupied hardly twenty seconds, and which greatly expedited the future steps of the operation. Turning my attention now to the flaps, I turned one up and the other down, by bold and rapid strokes of the scalpel, and thus exposed the main mass of the diseased structure. By the time this was accomplished, the patient was pulseless, and it was necessary to wait five or six minutes, and give him brandy and ammonia before he was sufficiently restored to enable me to proceed. I next dissected the tumor from its attachments to the mucous membrane of the mouth, cut the mylo-hyoid, genio-hyoid, genio-hyoglossal, masseter, and pterygoid muscles, and separated the coronoid and condyloid processes, by means of a curved lever, with a strong handle, and a rounded, somewhat sharp extremity. This stage of the operation was executed with great facility; the ligaments and temporal muscle offering hardly any resistance.

Scarcely half a pint of blood was lost. The only arteries requiring the ligature were the external maxillary and the inferior dental, together with a small muscular branch. The external carotid and the internal maxillary were seen pulsating at the lower and back part of the wound, which was of great extent. The temporal artery was not divided.

The patient being still faint at the close of the operation, it was necessary to retain him on the table, and to administer stimulants, as brandy and ammonia, with sinapisms to the extremities. As soon as he was sufficiently recovered, I brought the parts together with six twisted and two interrupted sutures, filled the vacuities in the cheek with patent lint, and placed a compress upon the side of the face, in the line of the late tumor; the whole being supported by a roller carried round the head and chin. The patient was then put in bed and ordered one grain of sulphate of morphia.

In the afternoon, at seven o'clock, the patient was quite comfortable; he had no pain or fever, and swallowed with perfect ease. The morning after the operation I found him in fine spirits, and I learned that he had slept well during the previous night; he complained of slight stiffness in the throat, and had a little swelling in the face, but hardly any fever or thirst. He was ordered a dose of oil, which produced several slight evacuations during the preceding night, which he passed very comfortably.

The dressings were taken off on the 16th, when the wound was found united in its entire extent, except for about an inch at its centre, where it gaped a little. The parts here were brought together by two sutures, the four front needles taken out, aud nearly all the lint removed from the mouth. There was hardly any swelling or suppuration, and the patient swallowed well, and had a good appetite. The other sutures and lint were removed on the 19th; when the patient moved his tongue well, and performed deglutition with great facility. In a word, he was perfectly comfortable, and entirely out of danger.

On the 13th of June, Milton left Louisville on the steamer Mayflower, for Nashville. He was still feeble, but could walk several miles a day. The left side of the face was somewhat swollen and tender, especially towards the zygoma; and a small opening still existed about the centre of the cicatrice, which was considerably depressed, or sunk in towards the cheek. Through this opening a little saliva flowed during eating. A minute fistula, as big as a pin's head, also existed over the parotid region, and occasionly discharged a drop of saliva. The patient had an excellent appetite, and had some use of the right side of the jaw in mastication.

From a letter received from Mr. Ewin, dated July 30, 1852, I learn that Milton is in good health, and that his face is entirely healed and sound. "He is regaining his

flesh, and looks well, but is not able to bear hard labor, or exposure to the hot sun."

The tumor was carefully examined after removal, and was found to consist chiefly of a number of osseous cysts, of variable dimensions, and filled with a thin, turbid serum. None of the cells were occupied with encephaloid or scirrhous matter. When struck with the handle of a knife, the tumor emitted a sound like that of a dice-box. The bone was entirely sound in front, and also at the condyle and coronoid process.

Case 4.—A very genteel mechanic, thirty-five years of age, from the interior of the State of New York, consulted the late Professor Pattison and myself in November, 1850, for a fungous growth of the lower jaw, which he had first noticed not quite a year ago, and which had been removed some months before by a physician in the country. The tumor, which was of a pale reddish complexion, and of a dense, gristly consistence, sprung from the alveolar process, in the situation of the right molar teeth, the anterior of which was imbedded in its substance. It was about the size of an almond, and was the seat of a dull, heavy, aching pain, attended with some stiffness of the temporo-maxillary articulation, and more or less uneasiness in mastication. The integuments over the tumor were sound, and there was no perceptible external deformity. The general health was excellent.

The operation was performed on the 2d of November, at the Surgical Clinique of the University of New York, in presence of the medical class, and with the aid of Professor Pattison, the patient being under the influence of chloroform. The first bicuspid and last molar teeth having been removed, a horizontal incision, about three inches and a half in length, was made along the inferior margin of the body of the jaw, directly over the diseased part. The flaps were then dissected off, and the bone, which was excessively hard and dense, sawed through at the ramus

posteriorly and just behind the mental foramen anteriorly. The facial artery was necessarily divided, and this, with a small muscular branch, was the only vessel that was tied. The edges of the wound were then brought in apposition, and retained by four twisted sutures, a compress, and a roller. Hey's metacarpal saw was employed for dividing the bone.

The patient had no untoward symptoms after the operation, except a slight attack of erysipelas, and went home in about two weeks perfectly well. The fungous growth, previously alluded to, had its origin deep in the alveolar process, and seemed to be of the nature of epulis. The base of the body of the bone was quite sound.

Case 5.—Mr. S. W. S., of Allen county, Kentucky, aged 32, farmer, tall and slender, and of florid complexion, consulted me in April, 1851, respecting an affection of the lower jaw, which, as he informed me, he first observed three months ago. The first thing that attracted his attention was a soreness of the gum round the last grinder of the lower jaw on the right side, which was soon succeeded by a small, red, tender swelling, most conspicuous on the inside of the tooth. The tumor gradually increased in volume, and as it did so the soreness, previously complained of, entirely disappeared. As he has never sustained any local injury, he supposes that the disease had a spontaneous origin. He was always healthy until his seventeenth year, when he was seized with some obscure affection of the heart, accompanied with neuralgic pains in the left side, which continued for a few months, and then gradually left him. Since that period he has never been sick.

When the swelling was the size of the end of the thumb, which it attained in a fortnight after it was first observed, it was cut out by a physician in the country. The operation, slight as it was, was attended with a good deal of hemorrhage. In two weeks, the tumor, which was now

fully three times as big as before, was removed a second time, and along with it the last grinder, the root of which was much diseased. There was again a considerable hemorrhage. This operation was followed, in a few hours, by swelling of the corresponding side of the neck and face, which, rapidly increasing, became excessively painful, and at length terminated in suppuration. The discharge of matter was copious, and lasted for several weeks. When the patient arrived in town, April 23, the parts were still hard and rough, and there was a small fistulous opening just below the angle of the jaw, giving vent to a thin gleety fluid. The tumor, at this time, formed two large cylindrical masses, one on each side of the dental arch, of a pale red color, devoid of pain, elastic, and of a firm consistence, extending from the ramus of the jaw to the first bicuspid tooth. The cheek was sound, and was not in the least protruded by the morbid growth, which, however, considerably impeded the movements of the jaw, mastication, and articulation.

Satisfied, from all the circumstances of the case, that nothing short of excision of a portion of the jaw would hold out any prospect of permanent relief, I performed the operation on the 27th of April, Drs. Goddard. T. G. Richardson, and Thomson assisting. The anterior bicuspid being extracted, and chloroform administered, an incision, about four inches in length, and slightly curved in its direction, was made along the base of the jaw from before backwards; the parts, which were very hard, and bled quite freely, were then dissected up, and the bone thoroughly exposed at the seat of the tumor. Not knowing how deeply the disease extended, and anxious to save as much of the bone as possible, I limited myself, in the first instance, entirely to the alveolar process, but after it was removed, a careful examination rendered it plain enough that a portion of the morbid growth had been left behind. I determined, therefore, to saw the

jaw through at the first bicuspid tooth, and just in front of the ascending ramus. Four ligatures were applied; among them, one to the external maxillary artery. which was inclined to bleed quite freely, as it could not possibly retract, in consequence of the indurated and condensed condition of the soft parts. The wound was dressed with seven twisted sutures, a compress, and roller. Upon inspecting it at the end of the fourth day, it was found to have united perfectly by the first intention. A little erysipelas made its appearance about thirty-six hours after the operation, but soon yielded to a dose of purgative medicine and the application of the tincture of iodine. No further trouble occurred, and the patient went home in about a fortnight after the operation.

In the latter part of June Mr. S. wrote me that he was doing well, except that he had some difficulty in masticating, and that there was still some swelling under the jaw. Early in September I heard from him again, to the effect that the gap left by the removal of the bone, was occupied by a tolerably large mass, quite dense and discharging constantly more or less pus. The tumor was steadily increasing, but was free from pain and soreness. At my request, he came to town a short time after; and on the 24th of September I operated upon him a second time, removing the whole of the new growth, which was about the size of a pullet's egg, and about three-quarters of an inch of the anterior extremity of the ramus of the jaw, from which the diseased structure seemed to spring. The incisions which it was necessary to make for this purpose, extended in a vertical and horizontal direction, from the outer part of the lower lip on the right side to the angle of the jaw, and were approximated with the twisted suture. In consequence, however, of the want of support in the cheek, a small part of the horizontal portion of the wound refused to unite; and it became necessary, afterwards, to pare the edges of the opening and to stitch them

together, to facilitate adhesion. Notwithstanding this, however, it was not until sometime in April, 1852, that the consolidation was completed. On the 2d of July he wrote me that he was entirely well, and that there was not the slightest appearance of return of the local affection.

There can be no doubt that the tumor in this case was an epulis, commencing in the gum, or the gum and socket of the last molar tooth, and gradually extending to the alveolar process and body of the bone. Its reproductive tendency would mark it as malignant, and as likely to lead, at some future period, to disastrous results.

On the 21st of August last, Mr. S. wrote me that the disease was returning; and before I had time to answer his letter, he arrived in town. About three weeks before the date here referred to, a tumor began to form at the anterior extremity of the ramus of the jaw, which has rapidly increased until it is now the size of a pullet's egg, pressing against the upper grinders, and gently pushing out the posterior part of the cheek; it is of a rounded shape, very dense and firm, of a pale color, and quite tender on pressure. At times, sharp, stinging pains shoot through it. The front of the jaw is sound, and the general health excellent.

On the 31st of August, assisted by Drs. T. G. Richardson and Thomson, and my pupil, Mr. White, I excised the ramus of the jaw at the articulation. An incision, slightly curved in its direction, nearly four inches in length, and situated about nine lines in front of the ear, being carried from the zygoma to a short distance below the anterior extremity of the tumor, the flaps were detached from the subjacent structures, and the bone denuded with the instrument described in a previous part of this paper, aided by the point and handle of the scalpel. In the effort of separating the coronoid and condyloid processes, a pretty copious hemorrhage ensued, which

was increased, for a few seconds, by the pressure which I directed to be made upon the common carotid artery, under the supposition that it was arterial in its character, which, however, was found not to be the fact. As soon as the bone was detached the bleeding ceased, and not a single ligature was required. Nearly a pint of blood was lost; and at one moment a considerable quantity passed into the patient's mouth, causing some embarrassment of breathing. The edges of the incision were carefully approximated by five twisted sutures, and supported by a compress and bandage; a small roll of patent lint, wet with a strong solution of alum, having been previously inserted into the upper angle of the wound.

The bone, examined after its removal, was found to be completely divested of its periosteum, and even of the cartilaginous covering of its condyle. Not a fibre of the masseter, temporal, and internal pterygoid muscles was left. A portion of the insertion of the external pterygoid alone was visible. With this exception, the bone, in the healthy part of its extent, was as clean as if it had just come out of the macerating tub. The morbid growth, scarcely as large as a pullet's egg, and of a dense, firm structure, partly fibro-cartilaginous, and partly osseous, was attached to the inner surface and anterior extremity of the bone, the latter of which was unnaturally thin and somewhat notched. The external surface, neck, condyle, and coronoid process were all sound; but the inner surface, nearly as far back as the dental foramen, was slightly roughened and thickened, from the effects of disease. The length of the bone, measuring from the condyle, was two inches and five-eights.

Some swelling of the cheek and lip supervened; but the pain was very slight, and the patient had, at no time, any difficulty in swallowing. The sutures were removed at the end of the fourth day, when the wound was found to have united by the first intention. The lint, at the

upper angle of the wound, was allowed to remain till the sixth day. Two weeks have now elapsed since the operation, and the patient is perfectly well, having never had a single bad symptom. His general health is excellent.

ART. II.—*A Case of Doubtful Paternity.* By W. L. SUTTON, M. D., President of the Medical Society of Kentucky.

What distinguishes a child of pure white blood from one tainted with that of the negro?

About a fortnight ago a child was brought to Georgetown by its reputed father, accompanied by his physician, a gentleman of some forty-five or fifty years, for examination by the physicians of the town, partly with a view of ascertaining whether any course could be suggested which would insure the continuance of its life, and partly to silence some neighborhood talk which had arisen on account of its color. The physician believed that the color of the child was occasioned by the foramen ovale remaining open; in proof of this, he alleged that when the child cried he became much darker—decidedly blue—and thought that the imperfect aëration of the blood consequent upon the patent condition of the foramen, was sufficient to account for the permanent dark color of the skin. Another physician who had seen the child at two months old, believed then that he labored under inflammation of the brain, and, from some cause, suspected the foramen ovale was open. Among other things he suggested a trial of the advice of Prof. Meigs, respecting position.

The moral testimony in the case was, that up to the birth of the child, the mother had been entirely above suspicion. In fact, she was considered a very modest woman. She was said to have fair complexion, light hair, and blue eyes. The husband, who accompanied the child,

had nothing remarkable as to complexion; hair of the ordinary brownish color. His mother reported to be very dark, with black hair.

The child is a boy of four months, with black, straight hair, the fine hair on the forehead black; rounded forehead, broad nose, particularly expanded at the alæ, skin dark, yet not darker than purely white children are sometimes seen. Near the extremity of the coccyx, and rather to one side, was a spot, oval in shape, about three-fourths of an inch long, decidedly dark. No other dark spot perceived. There is a popular notion that when a child is tainted with African blood, the scrotum and a streak down the back are always dark. Nothing of that kind existed.

Three physicians (the one who had seen the child at two months, among them,) were unanimous that the color did not depend on cyanosis, and that there were appearances about the child of very suspicious tendency; but declined any expression as to admixture of blood, without a better acquaintance with the relatives of the husband and wife. One, however, could not believe that two parents of the temperament of the husband and wife, could produce a child of that color. Another thought it possible that with a grandmother decidedly dark, a child might be even that dark; but could see no good reason why a child purely white should have such a nose. The third thought that although the appearances were suspicious, they were not enough so to give an opinion unfavorable to a woman as free from suspicion as the mother had been.

The family physician, who had unshaken faith in the chastity of the woman, expressed his unfeigned astonishment that any hesitation should be felt in giving an opinion tending to exculpate the mother from all suspicion. Not satisfied with the result of this consultation, he procured the attendance of some five or six others at the residence

of the parties. These last, I understand, took very much the same view of the case that had been taken in the consultation.

Subsequently it is reported that the woman acknowledged that she had had occasional connection with two negro men in the neighborhood.

As this is a point which has been but little investigated, (some little discussion in Beck's Medical Jurisprudence being all that I am aware of,) it may not be amiss to make a few comments. Many persons think nothing would be easier than to "tell a white child from a black one;" but like a great many other things, the more it is studied, the more difficulties start up. From the nature of things, our European brethren give us no authority upon this point. Beck, vol. 1, p. 485, gives us a case which occurred in New York. In this case a mulatto woman swore a child to a black man. On trial, when the child was one year and seven months old, it appeared in evidence "that the child was somewhat dark, but lighter than the generality of mulattoes; and that its hair was straight, and had none of the peculiarities of the negro race. Many of the most eminent members of the medical profession were examined, and they all, with the exception of Dr. Mitchill, declared that its appearance contradicted the idea that it was the child of a black man. Dr. Mitchill, for various reasons, placed great faith in the oath of the female, and persisted in his belief of its paternity, although he allowed its appearance was an anomaly. The Mayor (Hon DeWitt Clinton) and the Court decided in favor of Whistelo," *i. e.* that the child was by a white man.

Dunglison ridicules the opinion of Dr. Mitchill. But Mitchell was altogether as respectable in the profession, and probably as cautious as Dunglison. Beck comments upon the above case as follows: "It will not do, however, to extend this rule too positively with what may be called *mixed breed.*"

Parsons gives an account in the Philosophical Transactions of a black man married to an English woman, of whom the offspring was quite black. In a similar case, the child resembled its mother in fairness of features, and indeed the whole skin was white, except some spots on the thigh, which were as black as the father.

White, in his work on the *Gradation of Man*, mentions a negress who had twins by an Englishman. One was perfectly black, its hair short, woolly, and curled; the other was white, with hair resembling that of an European.

So, also, Dr. Winterbottom knew a family of six persons, one-half of which were almost as light colored as mulattoes, while the other was jet black. The father was a deep black, the mother a mulatto.

"The offspring of a black and white," says Lawrence, "may be either black or white, instead of being mixed, and in some rare cases it has been spotted."

I have made the above extensive extract from Beck, (which is all he says on the subject,) because of the liability of such cases to arise in this community, where we have not only the white and the black races, but every conceivable degree of mixture. In a note to the above extract Beck adverts to the fact that, "At birth it [the new-born black infant] cannot be always distinguished from the white; its hair has not yet its peculiaa make, and we can only notice the tendency to dark on some parts of the body. In a few days, however, the change commences on the countenance, and gradually extends over the body." This is rather too positive. In many cases, a child of purely black parents is so white at birth as to exhibit no "tendency to dark" on any part of the body; and like other changes, this sometimes takes place much more slowly than in others.

There is much truth in the extracts made by Beck above, as careful examination of our mulattoes will shew. In this town there is a family—the father half white, the

mother three-fourths—whose children vary very much in color. Some being pretty good samples of the negro, and others, at five or six years old, not only as white as most white children, but having straight and light-colored hair.

The spots spoken of in the above extracts are certainly rare; nor do I know to how much consideration they are entitled. There was one on the child which gives rise to these remarks. On the other hand, without being able at this time to refer to any particular case, I am certainly under the impression that I have seen persons, entirely free from suspicion of admixture, who had a dark spot on some part of the body. I, some time since, owned a negress who clearly had no white blood in her, yet she had a large spot on the forehead and temples greatly darker than her skin in other parts.

The hair, although a very important feature, is not conclusive in determining our judgment. It does not necessarily begin to assume its distinctive character in a few days, as we might infer from the expression of Beck. In half-breeds, generally, it is only curly, and not knappy, as in the negro; frequently it is no more curly than occurs occasionally in persons purely white; whilst again it is as knappy as in the negro.

It seems that no reliance can be placed upon the popular lar notion that the scrotum and skin over the spine are dark in children having an admixture of African blood. I have examined several children of six to eight years old without finding it in any of them; they were, however, more than half white—some two-thirds, some three-fourths.

We must pay some attention to what is called moral testimony, but that, like other considerations, must be watched. The above case shows how guilty a woman may be, and yet escape suspicion.

In stating facts, I have gone upon the presumption that they were really as they appear. For instance, in the

family referred to as living in this town. Some of the children *may* be by fathers purely white, and others by those wholly black. I can only say that no suspicion attaches.

The case cited in which an Englishman impregnated a negress, one child being white and the other black, may be as reported, yet we have a case reported of a woman in Virginia having connection with her husband, and very soon afterwards with a negro man, and becoming impregnated by both.

There is, perhaps, no subject connected with our profession which presents more knotty points than this. For this reason—and because no one knows when he may be consulted upon such a case, and further, as we have so little authority on this point, and because the country South of Mason & Dixon's line could and ought to furnish the facts and authority upon this subject—I have thrown together the above facts and suggestions, in hope that they may be a means of drawing others out on the same subject.

GEORGETOWN, Aug. 18, 1852.

REVIEWS.

Art. III.—*The Principles and Practice of Surgery.* Illustrated by three hundred Engravings on Wood. By William Pirrie, F. R. S. E., Regius Professor of Surgery in the Marischal College and University of Aberdeen; Surgeon to the Royal Infirmary, &c., &c.

We referred very briefly to this work in our last number, and gave the impression that a superficial examination of it had made upon our minds. A more extended and careful inspection of its varied contents has confirmed that favorable impression, and we have no doubt that it is destined to become, in a very short time, one of the most popular text-books on Surgery extant. This American reprint of the English edition has been improved by the editor, Dr. Neill, who has not only increased the number of illustrations, but added several new articles to the original of peculiar interest to the American student. The subjects treated of in the volume are, inflammation and its results; erythema and erysipelas; wounds; burns; fractures; injuries of the head; dislocations; affections of the osseous system; diseases of joints; curvatures of the spine; talipes; affections of the arteries and veins; hernia; wounds of the abdomen; calculous disorders; affections of the testicle; affections of genito-urinary organs; amputations and resections; deligation of arteries; affections of rectum; affections of the eye and its appendages; affections of nose; affections of mouth, throat and windpipe; tumors; and affections of

breast; to each of which a chapter is devoted, twenty-five in all. The illustrations are exceedingly well given, and impart a higher value to the work. In a word, it strikes us as being precisely the work wanted by students on this subject. We proceed to notice a few points discussed by the author at such length as our limits will permit.

In reference to the origin of erysipelas he remarks, that while it comes on in many instances without any obvious cause, it often spreads by means of contagion—a remark, the truth of which, though the French authorities generally deny it, will be questioned by few American physicians of experience. It is curious to observe, indeed, how the doctrine of the transmission of disease by this agency is gaining ground in our country. A few years ago, the popular doctrine of our schools was, that only small-pox, syphilis, and a few other complaints, were unequivocally contagious. The communicability of measles, scarlatina, erysipelas, &c., by this method, if not openly denied, was strongly questioned by our highest authorities. But at the present time few teachers are to be found who entertain doubts on this point; and cholera, typhus and typhoid fever, seem to be passing rapidly into the same category.

Mr. Pirrie speaks of a "periodic erysipelas," of which, we presume, the experience of most of our senior brethren has furnished them with examples. He says:

The peculiarity of this form is not merely that it returns, but that it is sometimes strictly periodical, returning more frequently (so far as my observation has enabled me to form an opinion) to the parts which were previously attacked; in some instances it has been found to be periodical in return, and universal in extent—that is, extending over the whole body; an example of which occurred in the experience of Mr. Maul, of Southampton, in the case of a lady who had several attacks at intervals of two years. In some instances the attack is monthly, at the

to be justifiable under the following circumstances:—

1. In simple fracture with depression, provided the symptoms persist after the use of depletion, purging, and other alleviating remedies.

2. In compound fracture with depression, without symptoms of compression.

3. In punctured fracture, without symptoms of compression.

4. When the symptoms are very urgent, and the surgeon thinks he has good reason to believe, that they are caused by blood or purulent matter underneath the cranium and above the dura mater, or by fracture with depression of the inner table.

In each of the first three conditions, it is considered necessary to adopt proceedings for raising the depressed portion of bone to its proper level. If, on exposing the fracture, it is found that this cannot be done by means of the elevator, which is often the case, in consequence of the fractured portions being so related that it is impossible to insinuate the extremity of the elevator underneath the depressed portion, then it is advisable to remove a small portion of the cranium by means of the trephine, for the purpose of allowing the introduction of the elevator by which the depressed part is to be raised. The instrument used by the ancients was the trepan, and the operation was called trepanning; the instrument now used is called the trephine, and the operation, trephining.

Such are the views now entertained in these latter and better times of surgery, as to the conditions which justify and require the operation of trephining. We find, however, from the history of surgery, that a very different doctrine prevailed in former days; that so great was the rage for trepanning among the ancients, that the very slightest fissure, or even the mere suspicion of one, was considered to be sufficient warrant for the operation; that they trepanned in all fractures, whether attended by depression or not, whether attended by symptoms of compression or otherwise; that they operated when bad symptoms were present, to remove them—when absent, to prevent them; so that they elevated every depression, trepanned every fracture, and, in operating on a longitudinal or a radiated fracture, they trepanned along the whole of its course, so as to saw it out; and did not allow

a single fissure of it, or rima, as they called it, to escape. In operating for the removal of a coagulum, they made as many openings as would uncover, if possible, the whole of it; and, says Ravaton, "I have seen surgeons so infatuated, so desperately bent on discovering abscess of the dura mater, that, after applying six crowns of the trepan, they would, and I verily believe have, pulled away all the remaining bones of the cranium, had not their patients been delivered by death from such operations." All this was done, they said, to remove danger. It is almost incredible to what a disgraceful extent this fashion for trepanning was allowed to outrage common sense; and it is difficult to imagine how they could believe a fracture to be so dangerous, and their operations so safe. To show the extent to which trepanning was carried, Mr. John Bell gives the following quotation:—"Godifredus, Chief Surgeon of the States of Holland, mentions with particular exultation the performance of this operation by his friend, who trepanned the cranium of the Count of Nassau twenty-seven times, and that the fact might be established on indisputable authority, he made the said Count of Nassau, after he was recovered, write the following curious certificate, on the 12th day of August, 1664: 'I, the underwritten, Philip Count Nassau, hereby declare and testify that Mr. Henry Chadborn did trepan me in the skull twenty-seven times, and after that did cure me well and soundly.'" These practices, and the numerous inventions of instruments for cutting the skull, are sad monuments of the surgery of past ages. In later times, the Royal Academy of Surgery in France revived and defended the doctrine that all fractures of the cranium ought to be trephined. It does seem surprising that this body of men, convened for the purpose of ascertaining the principles which ought to regulate the practice of our science, and to whom surgery is in other respects so greatly indebted, shonld, by giving the sanction of their high authority to so dangerous a doctrine, have led the young members of our profession to adopt very dangerous rules of practice. The unfortunate results of the operation were so numerous, that the celebrated Desault, one of the greatest ornaments of surgery in France, forming his opinion from what he saw at the Hôtel Dieu, strongly condemned the practice, and, in the latter part of his life, entirely dis-

carded it. The doctrine of the French Academy met with a most powerful advocate in this country, in the late Mr. Pott, who, with all the great talent and decision for which he was so eminently distinguished, maintained the doctrine of trephining in almost every fracture, to prevent as well as to remove bad symptoms. He states that fracture of the skull in many cases is not attended with any symptoms actually demanding this operation at the moment; but although there may be no symptoms denoting affection of the membranes, or of the brain itself, yet inflammation in those parts will, in consequence of the fracture, come on at a more or less remote period, and that, therefore, recourse ought to be had to the operation. Mr. Pott, in speaking of the doctrine of trephining, says, "I am as much convinced of this as of any fact which repeated experience may have taught me," and throughout his writings on that subject, he gives his opinion with so much decision, supporting it by cases and arguments, that his views cannot fail to have produced a very decided impression. Notwithstanding, however, all the eloquence and talent with which he supported his views, the doctrine he taught is now abandoned; and in these later and milder times of surgery, operative procedure is considered justifiable only under the circumstonces mentioned at the commencement of this section.

This extract will recall to the mind of the reader some excellent remarks on this subject by Dr. Hardin, in the March number of this Journal, in which the ground was taken that trephining should be performed in cases of depression, whether symptoms of compression were present or not. We entirely concur in the views of Dr. Hardin, and while we should deprecate that reckless resort to the instrument which so disgraced early surgery, we hold that the timidity is to be avoided, which, to shun a slight danger, subjects the patient to the peril of epilepsy, mania, and fatuity. Depression cannot be continued without imminent risk of these frightful consequences.

On the subject of dislocations, Prof. Pirrie treats fully, clearly, and ably. Referring to the auxilliaries in the re-

duction of dislocated joints, he says, "Chloroform is the most powerful, and it is now invariably preferred. It prevents pain, facilitates reduction, renders comparatively little force necessary, thereby diminishing the danger of injuring texture, and it leaves no permanent weakness of system."

In his chapter on "affections of the osseous system," he gives an account of periostitis, the idiopathic form of which was first clearly described by Sir Philip Crampton. The exciting causes of this form of the disease, are, "atmospherical influence and mechanical injury; the predisposing causes, a feeble, debilitated state of body, induced by mental anxiety, or long-continued derangement of the digestive apparatus." As to treatment, he remarks:

In the acute form, the constitutional treatment consists of low diet, general depletion, saline purgatives, diaphoretic medicines, and such means as are capable of procuring resolution. The local treatment includes quiet, an attitude favorable to the reflux of the venous blood, leeches, warm and emollient applications, as fomentations, poultices, and other antiphlogistic means. Free division of the periosteum should be employed, according to some, only when other treatment has failed. Professor Miller objects to free direct division if suppuration be not present, and recommends a valvular division of the inflamed periosteum. Professor Syme says, "The mode of treatment depends upon the intensity of the symptoms. When they are very violent and attended with smart fever, the most effectual practice is to make a free incision through the inflamed parts down to the bone. When less severe, no benefit is derived from this proceeding."

When chronic, it is to be treated by internal alterative remedies, as hydriodate of potass, of which ten grains should be given during the day; or, when this fails, the bichloride of mercury, for, singular as it may seem that, this mineral, which is a predisposing cause of periostitis, should prove a remedy, it is, nevertheless, "an ascertained

fact." Upon this point Mr. Lawrence is quoted by our author to the following effect:

I have seen, in so many instances, the pain in that disease continue unrelieved, in spite of the pretty active employment of local antiphlogistic means, in spite of mild mercurial treatment, and have so constantly found it yield only to the full influence of mercury on the system, that I own myself to be at a considerable loss to the opinion entertained by many, that inflammation of the periosteum and affections of the bone are actually brought by the use of mercury. It seems to be very inconsistent that one and the same medicine should be capable of decidedly relieving inflammation of a certain texture. I think I formerly had occasion to mention that I did not coincide with the opinion of many, that those states were produced by the mercury, and certainly, when speaking of inflammation of the periosteum, whether arising from syphilis or not, I do not know of any means so capable of relieving the disorder as mercury.

The joints are subject to many singular departures from a healthy condition, and illustrations of strange morbid action are given in the chapter which treats of these affections. It is a highly instructive chapter. The editor adds a section on 'hysterical affections of the joints," which we copy:

Hysterical females often complain of great pain in the joints, which might be mistaken for some real and dangerous disease of the part. According to Brodie, "At first there is pain referred to the hip, knee, or some other joint, without any evident tumefaction; the pain soon becomes very severe, and by degrees a puffy swelling takes place, in consequence of some degree of serous effusion into the cells of the cellular texture. The swelling is diffused, and in most instances trifling; but it varies in degree; and I have known, where the pain has been referred to the hip, the whole of the limb to be visibly enlarged from the crista of the ilium to the knee. There is always exceeding tenderness, connected with which, however

we may observe this remarkable circumstance, that gently touching the integuments in such a way as that the pressure cannot affect the deep-seated parts, will often be productive of much more pain than the handling of the limb in a more rude and careless manner. In one instance where there was this nervous affection of the knee, immediately below the joint, there was an actual loss of the natural sensibility; the numbness occupying the space of two or three inches in the middle of the leg. Persons who labor under this disease are generally liable to other complaints, and in all cases the symptoms appear to be aggravated, and kept up by being made the subject of constant anxiety and attention."

Treatment.—Unfortunately for the patient, this affection is sometimes treated for a diseased or injured joint by antiphlogistic measures, which necessarily aggravate the symptoms. Constitutional remedies are those to be relied on. All of the functions are to be restored to a healthy condition, particularly the menstrual and digestive, since these will be usually found at fault.

Hygienic remedies are more valuable than local. "The patient should have fresh air, generous living, and plenty of occupation for mind and body; she should be encouraged to take exercise, notwithstanding pain and weakness; and to resume, as far as possible, the habits of a healthy person." The shower-bath and frictions of the skin will improve the capillary circulation. Tonics, such as quinine, valerian, and iron, will be found most valuable where there is debility. Brodie has also found benefit in the use of assafœtida injections, and in the enveloping the joint with a plaster composed of equal parts of the extract of belladonna and soap plaster.

Curvatures of the spine forms the subject of the next chapter, and in this connection some interesting physiological remarks occur:

It may be regarded as a general law, that of two functions, voluntary motion and sensation, the former is almost invariably first removed, and the latter first restored; the rationale of which is, that the anterior columns of the spinal cord, which give off the anterior roots of

the nerves, by which they preside over voluntary motion, are nearer to the seat of the disease, and therefore more exposed to pressure than the posterior columns which give off the roots presiding over sensation. Although pressure on the spinal cord is usual in angular curvature, it is surprising how nature, even in some cases where the destruction is very great, yet continues to maintain the integrity of the vertebral canal, so as to prevent the cord from being compressed. Of many examples of this remarkable fact, I shall only refer to the following: Mr. Stafford mentions the case of a child, in whom, though the bodies of six dorsal vertebræ were destroyed, and the angle of the curve was very acute, paralysis did not occur. Professor Cruveilhier gives the particulars of a case in which the bodies of five dorsal vertebræ were completely destroyed; where the fifth dorsal vertebræ rested on the eleventh, the two becoming anchylosed, and at a very acute angle; and yet the medulla was preserved free from pressure. I have at present under my care a girl ten years of age, in whose case the bodies of the fourth, fifth, sixth, and seventh dorsal vertebræ must be entirely removed; an abscess is formed, and is pointing about the middle of the seventh rib; and judging from the appearance of the spine behind, the parts above and below the seat of the disease must be for a short distance almost parallel with one another, so abrupt is the curve; and still the patient is as yet quite free from any symptoms of compression of the spinal cord. The only explanation given of such cases is that the process of destruction must have been very slow, and the deviation from the natural form extremely gradual. Mr. Stafford remarks: "The completeness and incompleteness also of the symptoms very much depend upon the rapidity with which the curve takes place. If the destruction of the bodies of the vertebræ has been very quickly effected, the paraplegia is usually more complete; but, if it has been slow in its progress, the paralysis below is often very imperfect."

On the treatment of these deformities he has the following remarks:

Any attempt to remove the curvature would be most

injudicious. Anchylosis is the only favorable termination to be hoped for, and therefore the object aimed at in treatment should be, to place the patient under circumstances most likely to conduce to that result. With that view, it is indispensable, first, to keep the patient in a recumbent position, so as to remove from the diseased parts the pressure of the superimposed weight, and to preserve the parts as much as possible in a state of perfect quietude in that position; and secondly, to use all means, judicious and available in the circumstances of the case, for maintaining the general health. In some cases local remedies are highly beneficial.

That it is necessary to confine the patient to the recumbent position, does not admit of question, for it is evident that the superimposed weight pressing on the diseased part not only acts as a source of irritation, but must tend to increase the curvature; and it can only be effectually removed by placing the body in the horizontal position. And that any effort which nature may make to effect anchylosis may not be defeated, it is further necessary that the parts should, as much as possible, be prevented from being moved upon each other. Another advantage which results from preserving the parts at perfect rest in the horizontal position, is, that the removal of the irritation, caused by the superincumbent weight, from the diseased parts, diminishes the danger of the formation of abscess, which (as formerly stated) is a most unpromising occurrence, and must induce the gloomiest apprehensions as to the ultimate results. One of the best means for fulfilling the above indications, is, to place the patient in the supine position on Earle's bed, which, besides other advantages, rendering it very convenient for this part of the treatment, allows the relative position of the trunk and limbs with regard to each other to be slightly changed, without any risk of moving the diseased parts on each other. The slight change thus allowed, renders the confinement to the recumbent position much less irksome than otherwise it would be. As an additional precaution from preserving the diseased parts from any movement, it is, in many instances, advisable to apply splints on each side of the spine. The splints, in such cases, must suit the shape of the parts to which they are applied. Some recommend the patient to be placed in

the supine posture, but others give the preference to the prone position, because in that attitude the superimposed weight is more effectually removed—there is no risk of heat and irritation from pressure—it favors the return of venous blood from the bodies of the vertebræ—and the approach of paralysis, it is thought, may be deferred, as matter will gravitate away from the medulla. This position is also very convenient when local applications are necessary, and in some cases the curve is so abrupt, that it is almost impossible, with every precaution, to keep the patient long on his back without producing irritation of the soft parts. But notwithstanding the above-mentioned advantages, I confess I have, in the majority of cases, found treatment conducted in the supine posture more satisfactory, and chiefly, I believe, from the diseased parts being more easily preserved in a state approaching to complete immunity from motion, than is possible when the treatment is conducted with the patient in the prone position, in which I have often been annoyed by finding it impossible to prevent the patient from moving the upper part of the spine by frequently moving the head and shoulders; and, as far as my experience goes, the supine position is preferred by patients. Rest, however, of the diseased parts, and the recumbent position, whether the body be prone or supine, are of the utmost importance from the very commencement of the disease, until a cure is effected by anchylosis. When it is believed that anchylosis has taken place, and the patient is allowed to resume the erect attitude, it is a judicious precaution to employ for some time an apparatus, such as that generally known by the name of the spine supporter, for removing the superincumbent weight.

The maintenance of the general health is another and equally important indication, but unfortunately some of the best means for fulfilling it are not compatible with the rest and the recumbent position which form essential parts of judicious treatment. The great importance of attending to the general health must be evident, when it is considered under what circumstances scrofulous deposits are most apt to take place in bone. In individuals of a scrofulous diathesis, insufficient nutriment or clothing, living in damp or cold and impure atmosphere, want of exposure to the sun's rays, mental depression, and any

cause of debility acting permanently or habitually for a length of time, have unquestionably an influence in exciting scrofulous deposits in bone, as well as in other textures. These considerations suggest the necessity, especially in scrofulous cases, of a digestible diet, living in a pure dry atmosphere, (the bracing air of the seaside being often highly beneficial,) exposure to the light of the sun, the cultivation of habitual cheerfulness, the proper regulation of the digestive apparatus, and the use of such remedies as from the particular circumstances of the case are best calculated to improve the general health. The tonic medicines generally found most useful are the preparations of iron. But as far as medicine is concerned, I believe the most important point is, to have recourse to those remedies which, from the particular circumstances of the case, seem most likely to preserve the digestive organs in a proper state. Besides these means, in some cases, local remedies are necessary; but the employment of them will depend upon the nature of the cause of the disease. If the disease depend upon scrofulous caries of the vertebræ, or upon softening with absorption without ulceration or caries, depletion would be worse than useless, and would tend to weaken the patient. In these cases, the surgeon must content himself with advising the recumbent position, maintaining the diseased parts in a state of quietude, and prescribing all suitable means for preserving the general health. In scrofulous caries, benefit will often be found to accrue from the early and very cautious employment of counter irritation, along with the treatment here alluded to. If the curve arise from inflammation of the bodies of the vertebræ, of their investing membrane, or of the intervertebral cartilages, slight local depletion by leeching or cupping at the commencement of the disease, and afterwards counter-irritation, are known to be highly beneficial. The repeated application of small pieces of blister to each side of the vertebral column at the seat of the disease has been found well suited for children, and caustic issues for adults.

A chapter is devoted by Mr. Pirrie to Talipes, the cure for which is one of the triumphs of modern surgery; for from the time of Hippocrates until a period within the

memory of surgeons now living, the treatment of those deformities consisted in the employment of mechanical pressure. The following history of the discovery is interesting:

In 1784 the first step was taken towards the present method, a physician of the name of Thilenius having suggested the division of the tendons affected with shortening; and his suggestion was carried into practice by Lorenz, a surgeon at Frankfort, in the case of a young lady affected from infancy with Talipes varus. The heel descended two inches after the operation, and the lady was able to walk on the entire sole. It was the tendo Achillis that was divided, and the operation was performed under the direction of Thilenius. The division, however, was effected by a large wound. The suggestion of Thilenius was also carried into practice at a subsequent period by Sartorius, whose method of operating, however, was liable to many objections, particularly because he made extensive incisions of the superimposed parts, exposed the tendon at the part to be divided, and after section of the tendon, immediately attempted to bring the foot to its proper position. The consequences were unfavorable, and such as do not follow the operation of section of the tendons as it is practised at the present day. Michälis suggested a different method of treatment, which consisted in a partial division of the shortened tendon, and bring the foot to its proper position immediately after the operation; he recommended section of the tendon to the extent of one-third of its thickness, by which means its strength would be materially diminished. He performed his first operation in November, 1809. Both Sartorius and Michälis recommended immediate restoration of the foot to its proper position, the one after complete, the other after partial division of the tendon; but neither of their methods of treatment met with general adoption or approval.

Delpech had the merit of conceiving and recommending some new and important principles of treatment. Having observed that after rupture of the tendo Achillis and some other injuries, the uniting medium admits of considerable elongation, it occurred to him that the same elongation

could be obtained after section of the tendo Achillis for the cure of Talipes, provided mechanical extension be employed before the uniting medium has acquired great strength and firmness. He recommended that the section of the tendon should be effected without division of the common integument over it; and in the instance in which he performed the operation, he made a wound of the common integument an inch in length on each side of the tendo Achillis by passing a scalpel between it and the deeper-seated structures; and then by a convex edged bistoury, he divided the tendon from before backwards. He recommended that after the operation the cut portions should be preserved in apposition, by maintaining the foot in the distorted position by mechanism, until reunion of the divided tendon be effected—that careful and gradual extension of the uniting medium should then be made, until the tendon be of sufficient length; and this having been obtained, that the limb be kept in a proper position by means of an apparatus until the new substance has acquired sufficient strength. Delpech performed his operation in May, 1816, and although a cure was effected after a long period, and the patient ultimately recovered, yet there were so many discouraging circumstances, that he never repeated his operation. Although the mode of dividing the tendon recommended by Delpech is exceedingly objectionable, he certainly has the merit of having suggested some important principles in the after treatment. To Stromeyer, who performed his first operation in February, 1831, the praise belongs of having perceived what was objectional, and appreciated what was valuable, in the views of those who preceded, and of proposing and performing the safe and successful operation, and the mode of after treatment, which are now so generally approved of, and so successfully adopted.

The chapter on calculous disorders has been more enlarged by the editor than any other in the volume. He has copied freely from the late work of Prof. Gross on "Diseases and Injuries of the Bladder." It will be found to comprise whatever is most practical and important in relation to those interesting subjects. Western America

undoubtedly abounds more than any other quarter of the globe in calculous disorders. It is to our surgeons and physicians that the profession will look for the statistics which are to elucidate the pathology of these affections. It is in our region that the most remarkable feats have been performed in the extraction of urinary calculi. Western surgeons have unquestionably operated for stone in the bladder with greater successs than any others in the world.

In the chapter on affections of the eye and its appendages, Prof. Pirrie makes some interesting remarks in reference to strabismus, about which there was so much said in the journals a few years ago, and the operation for which, from having been so popular, seems suddenly to have fallen almost into disuse. We can see no reason for this sudden loss of popularity. No doubt the operation was sometimes unsuccessful. There are cases in which it ought never to be performed. Prof. Pirrie says:

> Cases of strabismus proceeding from gastric, intestinal, or uterine derangement, from general debility, or from affections of the brain, are all unfit for operation; as, also, are all cases in which the deformity is in consequence of opacity in the cornea, the distortion in such instances being an effort to remove the opacity from the axis of vision.

But in proper cases, as those "in which the strabismus is confirmed, and in which there is no evidence of an exciting cause calculated to keep up or bring back a disturbance in the equilibrium of muscular action within the orbit," there is no reason why it should not succeed. In all such cases, when correctly executed, it does succeed, and there are few operations which are attended with more gratifying results than that for strabismus.

Imperfect as this notice is, it must suffice for this admirable treatise. We could easily have filled our journal

with extrrcts from its pages which our readers would peruse with interest; but the space at our command is exhausted, and we dismiss it with the assurance that they cannot purchase a more valuable work on surgery.

ART. IV.—*Transactions of the Medical Association of Southern Central New York, at the Annual Meeting held at Owego,* June, 1852, —p. p. 94, 8vo.

These transactions open with the annual address of the President, Dr. James H. Jerome, who takes a passing notice of the principal systems of medicine which have successively prevailed. We find nothing in it worth copying.

The second paper is a report, by Dr. Green, on the vital statistics of Cortland county. The report closes with the following practical suggestions:

We are fully impressed with the conviction that country physicians, especially, could not better discharge their obligations to community, as guardians of the public health, than by an earnest recommendation of the simple use of *woolen flannel* next to the skin, and to a majority of persons during the whole year. We fully believe that it would, if generally adopted, very greatly lessen the frequency and severity of cases of congestion and inflammation of the digestive organs, and of the respiratory apparatus; and especially that the two diseases which often claim the highest number of victims in our county, viz: dysenteria and pulmo-tuberculosis, would be astonishingly diminished. If the abdomens of children were protected, during the summer months, with flannel, it would serve as a most efficient prophylactic against the bowel affections, to which they are so obnoxious. The common catarrhal affections which are often as prevalent in summer as in winter, might, in a great measure, be prevented by the use of flannel; or when they do occur, their cure might be rapidly promoted by a resort to the flannel wrapper, or by the simple expedient of lining the back of the shirt with a strip of flannel, thus protecting the spine from sudden atmospheric vicissitudes.

The third article is a very condensed and brief report on the vital statistics of Owego, by Dr. L. H. Allen; which is succeeded by a report, by Dr. F. Hyde, on the surgery of Cortland county. The following is the history of an accident which, for the want of judicious early treatment, rarely has as favorable a termination as in this instance:

Incised Wound Opening the Knee-joint.—The following case occurred under the observation of Dr. L. S. Keene, of McGrawville, a subjoined sketch of which he has furnished the committee. A lad of eleven years fell from a fence, striking his knee on the edge of a sharp scythe, cutting open and disarticulating the knee, leaving only the ham-strings, large nerves, and blood-vessels undivided. A small piece of the head of the fibula was shaved off, otherwise the wound was directly through the joint, exposing the entire articulation.

Treatment.—The limb was placed in the straight position, one of the articular arteries tied, the edges of the wound brought together, maintained by sutures and adhesive plasters, on which were applied a rattan and splints. Anti-inflammatory measures were pursued.—Two-thirds of the wound united by first intention; the remainder healed kindly, after which the parts were subjected to slight motion daily. In two months from the receipt of the injury, he could walk by drawing the limb after him. At the end of five months he regained the power of flexion and extension sufficiently to walk quite well. At this time there was considerable effusion in the joint, but it has been gradually absorbed.

The surgical report of Tioga county makes the next article, which begins with the following case of *cancrum oris:*

H. T., girl six years old, of scrofulous habit, has had occasional obstinate swellings about face and neck, since having the measles. Now convalescing from billious remitting fever. Oct. 23, 1851. Cried with tooth-ache,

fever gone, tongue clean, some desire for food, pulse 120, soft. Oct. 24. There is swelling of right cheek, which is red and painful; cold applications not tolerated; warm poultice of hops and vinegar applied, with considerable abatement of swelling and alleviation of pain.

25th. Much the same; appetite improving. 26th. Discoloration upon inner side of cheek, and gums ash color; tumefaction remains same. Applied strong solution Nit. Silver. 27th. Grangrene spreading upon inner side of cheek; teeth of that side loose; fœtid breath; tongue clean and moist; skin soft; appetite good; general appearances all good; mortification approaching the surface externally, showing itself in dark line, to the right and a little below the angle of the mouth—appearing like a bruise. Administer Quinine and wine liberally; kreosote; poultice of yeast and charcoal to cheek, changed frequently. Drs. Hoyt and Kiff called in consultation this afternoon; concur entirely in diagnosis and treatment.

28th. Mortification gradually extending; same treatment continued.

29th. Drs. Everett, Cole, and Woodworth called—same treatment continued, in accordance with the advice of consulting physicians; mortification still extending.—It has reached the size of a dollar upon external surface—constitutional sympathy very considerable.; appetite continued good—in short, the mortification continued to spread, the typhoid symptoms proportionably increased in severity until the 5th of November, when the patient died.

The next article consists of a report on surgery, by Dr. Burr, for the county of Broome. Of *Cancer*, the committee say:

One case of cancer, or encephaloid tumor, of the breas has occurred, or rather has terminated fatally within the year. Two operations for the removal of the diseased portion have been performed, but with no permanent good effect. The mother of this patient died a few years since with a simllar disease. Another member of the family is

now said to be suffering from the same affection, in its incipient stage.

One case of cancer reported by Dr. French, has terminated fatally. Its seat was in the parotid gland.

Spermatorrhœa.—Among these cases *three* have been treated after Lallemand's method. In two cases great amendment has taken place. In the third no perceptible amendment. Others have been treated in the usual way: by tonics and moral admonitions.

Two cases of Phlegmonous inflammation, and a case of Staphyloma, consequent upon purulent ophthalmia, are then given, and after these we have the following instructive case of imperforate anus, reported by Dr. Squire:

In November last I attended Mrs. Hemmenway in confinement. The mother got well, but the child died at the end of the fourth day, having had no evacuation of meconium, on account of imperforate anus. Perhaps the only instructive and remarkable circumstance connected with this case, is the fact that this malformation was not discovered till the post mortem examination was made. It is true that I was led to suspect this difficulty early, but my suspicions were all removed by the mother's statement that there was an external opening to the bowels, for she had introduced some soap, for the purpose of inducing an evacuation. The next day I made an examination myself and administered an injection, which came away immediately, but nothing followed. Afterwards I gave mild cathartics, and finally aloes, and the child died of course.

Externally, the place where the anus should have been, was perfectly evident to the eye, and did not differ in appearance from the natural anus where its sphinctor is firmly contracted. When the nates were separated, and the integuments put upon a firm stretch, a slight fossa or sulucs appeared where the natural opening should have been, and the complete occlusion was readily perceived. The color and appearance of this small fossa, were not the same as of genuine integument, but looked more as I should imagine the parts would look, if the natural anus had been obliterated by a cicatrix, resulting from a burn.

The bones of the pelvis having been separated at the pubis, and the condition of the internal parts examined, the rectum was found to terminate by a perfect cul de sac, in the hollow of the sacrum, the space of an inch or so intervening between the closed gut and the closed sphincter. A sheath of areolar tissue which invested the rectum at its unnatural extremity, was condensed into a small fibrous cord or ligament, which was firmly attached to the perineum, where the anus should have been. No other deformity existed. I doubt whether an operation in such a case as this would prove satisfactory. Especially would I object to any attempt to form an artificial anus in the groin or lumber region, as some authors advise, when the operation in the perineum fails. In such unfortunate cases, it is the surgeons duty to desist.

Dr. Jerome's report of a case of poisoning with oxalic acid, forms the next article. As the cases of poisoning by this article have not yet been much multiplied, we quote this, which is thus stated:

In March last, an Irish girl in the family of Mr. ——, of Trumansburg, through mistake, supposing it to be epsom salts, took a table spoonful of oxalic acid. On learning which, the gentleman with whom she lived sent immediately for a physician. Dr. Clark and myself were called. I saw her some twenty minutes from the time of her taking the acid, at which time she was retching powerfully, with frequent eructations and much anxiety of countenance. She complained also of a burning sensation in the region of the fauces and throat.

We immediately gave her largely of ipecac and sulphate of zinc, and as much warm water as she could be induced to drink, both of which were repeated at intervals of but a few minutes. We also irritated the fauces with the feather of a quill, and in about eight minutes vomiting was induced and the stomach thoroughly emptied of its contents. The water and ipecac were repeated until the stomach was evacuated three or four times. Calcined

magnesia was freely used during the time. The retching and vomiting ceased in about an hour. The other symptoms subsided, and the patient convalesced in three or four days, leaving no unpleasant traces of the poison, except a slight inflammation of the mucous membrane lining the mouth and stomach.

ART. V.—*Transactions of the Missouri Medical Association of the State of Missouri, at its Second Annual Meeting*, St Louis, 1852.

This is the second volume of Transactions of this Society, and a very creditable one is it to the industry and attainments of its members. Besides the minutes of proceedings, it consists of nearly a hundred pages of excellent matter, the annual address of the President, reports of Committees, essays, &c., and presents an appearance altogether respectable and tasteful. It is grateful to see such a production emanating from a part of the country where, in the recollection of some of the members of the Society, there could hardly be said to be a medical profession. Missouri, in every way, is making rapid strides to greatness, and our brethren there seem resolved that their profession shall keep pace with the commonwealth in commerce, wealth, and arts. Such a Society as they have organized reflects credit upon the State. We hope to find time to return to its Transactions, and to notice at length the carefully prepared and instructive reports and essays; but at present we can only commend them in general terms.

Dr. McPheeters delivered the annual address, of which his subject was, "*A thorough organization of the profession the only feasible plan of bringing about medical reform.*" Its tone and spirit are admirable, and it is written in an agreeable and graceful style. The reports on Surgery and Obstetrics, by Drs. Pope and Pallen, are excellent, and the same is true of all the others in the volume.

SELECTIONS

FROM

FOREIGN AND HOME MEDICAL JOURNALS.

On the Cancerous Degeneration of Warty Excresences, and their Treatment. By Richard G. H. Butcher, F. R. C. S. I., Examiner on Anatomy and Physiology in the Royal College of Surgeons in Ireland, Surgeon to Mercer's Hospital, &c., &c., &c.—*Mr. President:*—On a former occasion I had the honor of bringing before the notice of this Society (the Surgical) a paper on the relationship that is found to subsist between cancer and fungus hæmatodes; and illustrated this alliance by preparations and specimens—1stly, when the diseases co-existed together; 2ndly, where the one was consecutive to, or replaced by, the other; and 3rdly, where the two manifestations of disease were tinuous in the same tumor.

These facts are borne testimony to and established by the investigations of Langstaff (Med. Chir. Trans., vol. ix.) Cruveilhier (Anat. Pathol., livr. xxiii., Explanation of Plates, 5 and 6,) and others.

The observations which I now wish to lay before the profession are in reference to the cancerous degeneration of warty excrescences—an association which I do not think has met with all the careful attention from writers to which it is entitled. Entitled on two grounds—1st, from the frequency of the one as a sequence to the other;

and 2ndly, from the inveteracy of the connexion when once established.

The following cases will afford exposition of the various changes brought about, from the apparently innocent verruca to the cancerous ulcer, and this again to the contamination of the system and the springing up of encephaloid disease. To illustrate still further this subject, I shall lay before the Society numerous casts and drawings, accurate representations of the respective changes as they were effected in each individual case, and shall conclude with a few practical deductions from the premises obtained.

Case 1.—Anne Sullivan, aged fifty-two, applied to me for relief, in May, 1850, being then suffering severely from a large, painful, ulcerated tumor over the right eye. The history which she gave goes to prove that a wart about the size of a pea existed above the eyebrow ever since she was a child; that eleven months previous to her seeking my advice, it became painful and itchy; that she frequently tried to pick it away in little pieces, and often pulled long shreds out of it, the separation of which was always attended with sharp pain, lasting frequently for a lengthened period after, and usually with a smart flow of blood. About this time, too, the bulk of the swelling began rapidly to increase, with a red margin round it, and soon its appearance was altered in every respect from the original condition; the warty excrescence was cast off, and a small ulcerated surface, about the size of a shilling, lay exposed, which was elevated, hard, and circumscribed; yielding a thin yellowish discharge, and characterised by persistent pain of a pricking kind, subject at different times to various degrees of intensity. Day after day the tumor continued to enlarge, spreading its base by the accession of fresh nodules, which never rose to any greater height than half an inch above the surrounding healthy parts; the integuments thus appeared to ulcerate around, the destroyed part being supplanted with firm elevations, which, in their turn, coalesced, became convex, and in this way preserving the nodulated character of the entire surface. Thus the base extended widely in all directions —upwards on the forehead, inwards and beyond the mesial line, externally towards the temple, and down upon the cheek, and inferiorly so as to involve and depress the

upper lid, and compromise vision in the right eye. The extent of ulcerated surface measured round its circumference ten inches. This amount of disease, then, was hurried into existence in the incredibly short period of eleven months. The character of the sore was peculiarly cancerous, the surface being nodulated, hard, and firm almost as cartilage, yielding a discharge thin, yellowish, and watery; profuse in quantity, and emitting the peculiar odor so pathognomonic, and readily recognized by the surgeon accustomed to meet with this form of disease. Eight months after the commencement of the disease in the forehead, a tumor began to form in the upper part of the parotidean region; it gradually came on, at first attended with most severe darting pain through the ear, up along the side of the head, and forwards towards the face, and thus averting sleep for nights, even before there was any appreciable swelling. At this time, the pain, she states, to have been most agonizing; but it gradually declined as the bulk of the tumor was augmented. The size of this secondary growth obtains in magnitude about that of a split orange, and from its osseous boundaries its outline is not regular; it is also somewhat compressed transversely about its centre, and the upper part is more of an ovoid, while the lower portion is lobulated, and spread out. This cast, taken from the patient, most faithfully represents the appearances of the primary and secondary formations, and the coloration of each has been very carefully preserved. From a careful consideration of the phenomena attendant on this tumor, the rapidity of its growth, the character of the pain, the elastic sensation elicited by the touch, the color of its surface, I concluded it was of encephaloid formation, and referred it to that class. With anxiety I watched this creature for some time, and in about five weeks after the cast was taken, the most prominent part gave way, and a fungus shot out, never attaining beyond the size of a large fig, and emitting from its centre, at intervals of a few days, repeated arterial hæmorrhages; some of them to the extent of several ounces. She struggled on in this way for two months, when she died from the debility consequent upon those frequent losses, and I regret to say I could not obtain any dissection of the body. I examined the structure of the original tumor several times with the aid of

the microscope, and which most clearly proved its cancerous nature. A fine section of it showed the basis to be made up of fibrous tissue, having imbedded, as it were, in its structure numerous nucleated cells; many with nucleoli. The addition of acetic acid had no other effect than that of rendering more conspicuous the nuclei at the expense of almost the dissolution of the cell-wall. On subjecting a piece of the tumor to pressure, a juice could be expressed from it yielding an abundance of cells similar to those visible in the section, and by the addition of acetic acid were acted on with a similar result. Numerous granular bodies were also floated through the fluid.

Here is a drawing of the microscopic appearances of the primary tumor, showing the arrangement of the fibrous tissue, cells, and granular bodies, which I have adverted to. The next point to be cleared up in this case was, carefully to ascertain the nature of the secondary formation, the tumor behind the jaw, and to trace out the affinity between it and the antecedent true scirrhus by microscopic examination. After the tumor had burst and the fungus shot out, I introduced a grooved needle into its structure about an inch deep, then rotated it between the finger and thumb, and on withdrawing it the groove was loaded with the morbid product. This was not uniformly of the same consistence; some parts were harder than others. On placing a small portion of it under the microscope, every atom absolutely teemed with a profusion of nucleated cells, supported with the most delicate filamentous tissue. On examining some particles firmer than others, the cells were much the same, the only difference being in the compression of the cells; while those of the softer portions approximated more closely to a sphere. There were no caudate corpuscles present in this specimen.

Here is a drawing of the appearance of the cells, as represented under the same power as that used in the first picture.

Case 2.—Jane Murphy, aged seventy, a healthy-looking country woman, who had been mother of ten children, consulted me in January, 1849, for a small tumor situated beneath her chin in the mesial line. She mentioned that a wart had been there from childhood, but that within the last four months it had lost its form, the irregular sur-

face becoming smooth, its size larger, and extremely painful. She had been in the habit of frequently pressing the tumor, endeavoring to allay the pain, which often induced it to bleed, and then the annoyance in a measure subsided. When I first saw this patient, the tumor was about the size of a marble, smooth and polished on the surface, with a semi-transparency over it, of stony hardness and quite moveable. Taking these features into consideration, together with the characteristic pain, always of a lancinating nature, the altered aspect of the part, and the period of life at which it was brought about, I was led to the inference of malignant degeneration being set up in this change, and urged its immediate removal. Coexisting with this suspicious tubercle, there was a warty growth, larger than a pea, a little above the chin, and to the left side. This, she said, also existed from infancy, never gave her any uneasiness, and exactly resembled the one beneath the chin, previous to the alteration above noticed. I removed the tumor beneath the chin in January, 1849, by two elliptical incisions, their long axis from above downwards cutting far wide of the diseased structure, and deeper by several lines of the bed of the tumor. The wound inflicted readily admitted of being brought together from side to side, and the edges retained so by two fine needles and the twisted suture, compresses were placed beneath the ends of each needle with a double object, to bear off any undue pressure and to act as on the principle of the quill suture in supporting the lips of the wound at their very deepest line in contact, and thus taking the strain off the needles. So effectual was the snpport and apposition afforded that union by the first intention was constrained, almost through its entire track, the lower part only suppurating. In ten days after being cut, the wound was altogether healed, and the patient went to the country to her friends. Previous to her going home, I urged the removal of the wart above the chin, but to no effect; she would not submit to have it done. During nine months after the operation, she remained free from disease, and satisfied that a cure had been effected. About the end of this time, the wart, which had been permitted to remain, began to spread and get painful. The cicatrix resulting from the former operation became tender, tumid, and ultimately gave way by an ulcerated fis-

sure, which rapidly grew wider, yielding a profuse ichorous discharge.

The destructive action progressed for about a fortnight, when a fungus growth spread around the sulcus formed in the first instance, assuming the shape of a mushroom and the size of a crown-piece, its margin being turned over so as to rest upon the sound skin. She came up to town again for my advice, and I declined interfering by operation; the grounds of objection being chiefly founded on the presence of a deep sinus leading backwards towards the line of lymphatics, parallel and beneath the anterior margin of the sterno-mastoid muscle. Again, the root of the disease was struck so deep, and the width of the contaminating base so widely spread, that even the most expert operator could not be satisfied that the entire was removed. Palliatives were again ordered, and she returned to the country. For many months the disease very slowly increased, but the warty excrescence was very considerably augmented, its surface having ulcerated, and the same process spread its margin, until ultimately it joined the disease spreading upwards from beneath the chin, the two having coalesced and become inseparably united together. During the last four months still further changes have been added; not only has the original manifestation of the disease been progressive, but we have formed two additional tumors, situated one on either side of the neck, and in the line of the absorbents, manifestly of encephaloid nature. Their springiness and elasticity, their coloration, and above all, the microscopic examination of their contents on exploration, pointed to, and confirmed the opinion of, their being true cephaloma. In this miserable state she endured, the gravity of the symptoms having been greatly increased, pain giving rise to the most intolerable suffering, the features being haggard and pinched, and the skin of a dull ochry color, debility and emaciation having made rapid progress, and all the functions of the economy more and more becoming implicated in the deteriorating influence of the disease.

In this deplorable condition (in December, 1851), she went back to her family in the country, to await her final release from suffering, which, to all certainty, was not far distant.

Here is a cast accurately showing the condition of the

parts previous to operation in January, 1849; and here is a second, graphically illustrating the changes which have been brought about, from the period of nine months after the operation, when the disease appeared in the cicatrix, with all the progressive changes up to the present time (January, 1851), an interval of fifteen months having elapsed. The painting of each has been most truthfully executed.

I have also preserved these microscopic drawings, taken of the primary and secondary tumors as they appeared. Here is one representing the appearances of the tumor that first showed itself beneath the chin. It exhibits a number of true cancer cells, scattered everywhere through a fibrous basis. Some separate cells are also seen detached.

This second drawing shows the arrangement of the encephaloid tumors which sprung up beneath the mastoid muscles. The structure seemed entirely composed of myriads of nucleated cancer-cells, and very closely resembled the secondary formation in the case of Sullivan; inasmuch as there were no caudate corpuscles in this specimen either, and the cells were held together by the finest areola tissue.

Case 3.—Ellen Fitzpatrick, aged sixty-five, consulted me in March, 1850, for a large bleeding wart, placed above and behind the right ear; it was attended for some time before with repeated hæmorrhages. She said it had been there for many years, never created any annoyance until about six weeks before seeking my advice. She referred the great change which had taken place in it to a bruise occasioned by a water pail that she had been in the habit of carrying on her shoulder. Shortly after this "the wart became very sore," and soon the pain set in, of intense character, darting up along the side of the head, down towards the angle of the jaw, and represented by the sufferer as "indescribably severe." On examining the part, a highly irritable and inflamed base surrounded the tumor, which was about the size of a shilling, uneven on its surface, and elevated about half an inch; it was hard to the touch, and bled upon the slightest pressure from an ulcerated line partly round it and through its structure.

I removed this tumor with great care, cutting far wide

of the base, and as I thought most effectively. Two arteries sprung which required ligatures, and so free had been the excision that the edges of the wound would not permit of being brought together, yet it healed perfectly in three weeks by granulation, a soft yet polished cicatrix being left. For a period of eight months she continued quite well and exempt from all annoyance. After this time she began to complain of uneasiness behind the angle of the jaw on mastication; by degrees the part became tense, and then she felt a small tumor there. This at the time she believed originated from cold, and it did not alarm her, more particularly as she often relieved the urgent pain by repeated stuping. However, the swelling continued to increase so as to become perceptible, and when it attained such magnitude as to fill up the angle of the jaw, she began to suffer from the effects of paralysis of the facial division of the 7th nerve on the right side. Day after day the tumor extended itself, particularly in the direction of the site of the original warty excrescence. At this time she again sought my advice, and then the case was truly a lamentable one. A tumor, considerably larger than an orange, filled up the space between the angle of the jaw and the mastoid process, lost upwards towards the zygoma, passing downwards and encroaching on the neck, extending behind the ear, and implicating the structures attached to the occipital bone: uneven, projecting, and lobulated on its surface; fixed, irregular, and immoveable at its base. The color of the tumor was very remarkable and strikingly indicative of the condition so frequently associated with the proper circulation of the true cephaloma. Large veins traversed it in every direction, some of them lying, as it were, in grooves embedded on its surface; while again numerous vessels marked the coloration in a peculiar way, constituting what might be called a number of vascular spots, from which capillaries radiated in every direction for a short distance, and ultimately breaking up into a fine ramiform distribution.

Here is a cast taken from the patient at this time, which most accurately shows the position, form, and color of the secondary tumor, also the paralytic condition of the corresponding side of the face, from the implication of the motor portion of the 7th nerve with the morbid product.

The face is greatly distorted, and the right side very remarkable when contrasted with the other. Upon the forehead the integuments lie flat, smooth, and at rest, there being no wrinkles or motion as on the left side. A verticle furrow is placed nearly in the centre, dividing the bulging of the muscles on the left side from the uncontracted state of those on the right; and the slip of the occipito-frontalis muscle forms a remarkable prominence at the junction of the nasal bone with the frontal on the left side. The power of closing the eyelids of the right eye was lost; they remained always open. When asked to close the eye forcibly, although she made the attempt, there was not the slightest motion observed in the eyelids. When the eye was at rest, and the patient using the sound one, about half the pupil remained visible, but during sleep was completely concealed behind the upper lid. The conjunctiva of the eye was in a chronic state of inflammation, and exhibited through a lens a perfectly villous surface, permeated in every point with innumerable vessels. On close examination, the cornea looked dull, but at a little distance presented a borrowed brilliancy from the abundant flow of tears which were constantly secreted and pouring over the cheek. The lower eyelid drooped a little, and the mucous membrane lining it presented the same vascular arrangement as that covering the sclerotic coat. The right nostril lay flat, collapsed, and not distended on a deep inspiration, but rather closed together, and the nose pointed towards the left side. When she blew or attempted to whistle, the air escaped by the right angle of the mouth, the right buccinator not at all corresponding in action with the muscle of the left side, nor with that of the muscles of the chest and neck by which the air was expelled. In mastication, the food collected in the right cheek between it and the teeth, and the patient could not push it from its place without the assistance of the tongue, and frequently of the finger. The saliva constantly flowed out at this side, and when drinking, part of the fluid likewise escaped.

When the disease attained the size represented in the cast, it did not at all increase so rapidly as at first; and during the following thirteen months I had repeated opportunities of watching the course of the disease, a part of it ulcerated, a fungus shot out, and was attended by

small hæmorrhages. I regret to say in January, 1852, this creature took typhus fever from an individual in the same lodging house, and died on the tenth day. I could not obtain permission for an examination of the parts.

It may be said, the cases of cancerous degeneration which I have brought forward all occurred in patients of advanced life. In most of the instances which have fallen to my lot for observation, it was so; but I have also seen the change brought about in early age, which the following cases will testify.

Case 4.—Maria Williams, aged nineteen, a particularly handsome girl, of dark complexion, consulted me in February, 1849, for what appeared a very irritable wart, and situated on the forepart of the neck. She mentioned it had been there as long as she could remember, but that latterly it had increased and became very painful, which she attributed to the pressure of her dress. The tumor when I saw her was the size of a filbert, hard and irregular on the surface, which at the highest point was elevated about a quarter of an inch above the surrounding healthy skin. It was quite moveable, placed about the centre of the depression, situated above the sternum, and three quarters of an inch from its upper margin.

The patient suffered great uneasiness in her mind from the rapidity of its increase, and the "dread of cancer," as her mother had died of that disease, and great depression and annoyance from the constant pain present in it.

Mr. Tagert, whom I consulted in the case, agreed with me that it was better to remove the part,—a proposition in which the patient most readily acquiesced. I did so by two incisions, one on either side, and wide of its base, meeting above and below, and then, by a few touches of the knife lifted the tumor in its perfect integrity from the subjacent cellular tissue. The lips of the wound were brought together with two fine needles and the twisted suture. Union by the first intention was nearly accomplished on the fourth day, and in less than a fortnight the part was healed altogether. During the three years which have elapsed, I have several times seen this young woman, and up to the present date there has been no return of the disease, either in the cicatrix or elsewhere.

I regret to say I have mislaid the microscopic drawing of the tumor cut out in this case, which I made most care-

fully; and more particularly so as bearing on a question about which I think a good deal of uncertainty still exists. From my notes, however, the following are the particulars. The specimen yielded epithelial scales, in various conditions and stages; some compressed together, forming laminæ, whilst deeper ones assumed a somewhat square form, some of them a caudate shape, whilst around the base there were other cells which I at once pronounced to be cancer-cells. When separated and broken up they did not at all seem disposed to run together, they were nucleated, some with nucleoli, and which, on the addition of acetic acid, were rendered more distinct, and the cell-wall was nearly dissolved, while the other cells resisted its action with impunity. I am quite sure I was not led astray here by an appearance that frequently takes place—namely, the enlargement of the epithelial cells from endosmosis.

Mr. Wardrop records a very remarkable instance of this cancerous degeneration of a wart occurring in a subject much younger than in the case which I have just related. "I had an opportunity (writes this eminent pathologist) of seeing an example of a true cancerous sore in a girl about twelve years of age, and it is the only case of the kind which has come to my knowledge. It appeared on the lower part of the abdomen, and begun in the form of a black wart on the skin. The wart ulcerated, and the surrounding skin was gradually destroyed, so as to form an immense ulcer, having all the characters of a true cancerous sore, which at last destroyed the child."—(*Wardrop's Observations on Fungous Hæmatodes, p.* 189.)

Case 5.—The supervention of fungous hæmatodes, after the removal of a large wart from the inner side of the foot, is well exemplified by the following case which occurred in our hospital some time since:—Mary Murphy, aged twenty-eight, admitted into Mercer's Hospital, October, 1846, being the second time this year. In the preceding February she was received into the house for the removal of a large painful wart, fully the size of a half-crown piece, and situated on the inner side of the left foot. It occasioned her great pain, and was so irritable that even a stocking could not be worn over it, and it was deeply ulcerated round its base. At this time there was no evidence of internal disease, and the lymphatic glands

of the extremities were neither indurated nor enlarged; therefore Mr. Tagert removed the part, and without difficulty, for it had no deep attachment whatever; it was quite loose, and readily floated on the surface from the slightest touch. The wound quickly healed, and in three weeks she returned to the country. Her second admission, as above dated, was nine months after this operation, when she was received with far advanced encephaloid disease in the groin of the same side. The history which she gave of the tumor in the groin is as follows: For five weeks after her return home—that is, two months from the period of the operation—she was free from all disease; that exactly at this time "a kernel" appeared in the left groin; it continued to increase for a month, and attained the size of a small apple, when it remained stationary for a short time. Up to this period there was very little uneasiness in the part. After this the tumor began again to enlarge, with a "bursting sensation" in it. During the following months her sufferings were greatly augmented, the tumor widely extending itself in all directions, irregular and nodulated on the surface, and highly sensitive. At this time, too, just before admission, the most prominent part burst, from which she lost a quantity of blood. In this state, then, she was received nine months after the operation, the tumor being larger than the clenched hand, accompanied by darting pains occasionally through it; but she refers an indescribable sensation of tension being always located in the upper half of it, and here, too, was a black spot marking the site from which the hæmorrhage had proceeded a few days before.

November 10th.—Since her admission to hospital, the increase of the tumor has been most rapid; it is now enormous, measuring ten inches and a half transversely, and seven and a half from above downwards. Its color is also greatly altered, being now of a dark purple and reddish hue all over. Its surface is irregularly lobulated, and deprived of skin, with the elevations coated over by a semi-opaque fluid, and the depressions containing unhealthy watery pus. The constitution is sympathizing acutely with this mass of local disease. The pulse is never under 120; she has at intervals during the night profuse perspirations; her countenance is haggard and of a yellowish hue; and all s petite is gone. One point in the upper part of the

tumor is far darker than the rest, and from which point two ounces of venous blood trickled the evening before. There has been no return of the disease on the foot, but the cicatrix is very hard and firm.

13th.—There was hæmorrhage last night to about two ounces, but it was readily restrained by a few dossils of lint steeped in spirits of turpentine and finger pressure.

18th.—Had profuse hæmorrhage last night; she lost nearly a pint of dark blood; to-day she is greatly exhausted, and bathed in sweat; her pulse weak, yet throbbing, and 130 in the minute; the tumor is quite black and turgid from where the blood flowed last night, and all its lobulated and broken up surface seems a mass of sloughs; she does not complain of pain now.

19th.—Is much depressed to-day; at six o'clock in the evening bleeding began again, at first slowly, and was staunched by pledgets of lint dipped in muriated tincture of iron. In two hours after it broke out afresh, and was perfectly uncontrollable. At this time the bleeding was frightful, it issued out in large bursts from the pultaceous disorganized mass. When pressure was made over one point, it welled up as rapidly from under another lobe of the fungus, and so on until death threatened by hæmorrhage; she was waxy pale, with violent jactitation of the arms, profuse cold sweat over the entire body, screaming for the windows to be opened and the admission of air. In these efforts at length all motions ceased, and though there was no appearance of life, yet the blood continued to flow for a few seconds longer, when the pulse forsook the heart, and then death.

On examination of the body, a tumor as large as a small melon, of the same nature as that in the groin, filled the iliac fossa of the same side, intimately attached to the fascia, and implicating the muscles in this region. The iliac artery and vein ran through its base, and below Poupart's ligament the femoral artery and vein were surrounded by the encephaloid structure situated there. This pathological condition may account for the fact of the total uselessness of pressure over either of the trunks in arresting the fatal hæmorrhage. On slitting up the artery and vein through their entire extent as they traversed this diseased mass, I could not by the closest examination find any solution of their integrity. Vessels of considerable size-

both arteries and veins, however, could be discovered through the structure, with their opened up and patulous extremities. These were very numerous, and evidently the source from which the blood issued in such quantities. The patulous condition of the arteries, as well as the veins, I ascribe to the matting of the coats of the vessels with the surrounding tissues, and thus neutralizing their contractile power. The softer parts of the tumor, on section, exactly resembled the brain in a state of decomposition.

Case 6.—The late Mr. Palmer, of this city, had a case very analogous to the one just particularized, a short time before under his care, in Mercer's Hospital. The patient was a young woman only twenty-four years of age; she had a flat painful wart on the inner side of the knee; it was there for years, but having become very irritable and ulcerated, and bleeding from the least injury, she solicited for its removal; it was taken away by the knife, and the part healed favorably. She returned to the hospital in five months after; the glands in the groin of the same side being enormously enlarged, and all the structures in the inguinal region participating in the encephaloid degeneration. This creature died before the end of the seventh month after the operation, of repeated and profuse hæmorrhages.

Now the cases which I have given are examples of only one condition of the skin preparatory to ulceration and malignancy; that is, when there exists an indurated warty tumor, and this I conceive to have a cancerous tendency, *ab initio.* The small growth may be unproductive of inconvenience for years, until irritated, as illustrated in many of the cases which I have adduced; then the characteristic pain, sharp and lancinating, never entirely deserts it; ulceration sets in, making breaches round its base, and proceeds to the detachment of the warty surface. During this time, a thin fluid exudes from underneath; hard firm granulations are thrown up from an indurated base, not rising very high, yet presenting a mammillated surface, far denser than the interior of the projecting nodules. The destructive process which I have endeavored to describe and elucidate by the foregoing cases, presents to the inquirer two very striking characteristics, and essentially belonging to it—1st, that when

once the ulcerative process is set up, there is never any amelioration, ever so temporary, no attempt at cicatrization; and 2nd, the great liability of the appearance of encephaloid disease, either in the site of the original tumor or in the line of the absorbents, returning from its position. Here, then, are two marked differences as to the results between it and the condition to which the term *noli me tangere* is applied, and to the *destructive ulceration* most accurately described by Dr. Jacob. Of this latter disease, I present to the Society this highly painted cast to contrast with those I have already exhibited. It shows well the characters of the disease as recorded by that gentleman. In this instance, though nearly half the scalp was destroyed, though inroads had been made by the disease to a considerable extent on the side and posterior part of the neck, the ear nearly detached, large vessels exposed, coated by small granulations, and sealed up against the passage of blood—yet, I say, with this amount of ulceration and death of parts around, the neighboring glands did not participate in or suffer contamination.

In the cases Nos. 1, 2, and 3, the germ of disease lay, as it were, innocuous; its malignant tendency did not manifest itself until a very advanced period of life, at the respective ages of 52, 70, and 65: while in the cases Nos. 4, 5, and 6, it was ushered into existence at a much earlier age, 19, 28, and 24; while in Mr. Wardrop's case, the subject, a little girl, was only 12 years old.

It is remarkable, too, that once the ulceration was fairly established in the primary tumor, true encephaloid disease rapidly sprung up either in its site or in its immediate locality, with the exception of case No. 4, successfully extirpated. Again, in every instance which I have recorded, all the changes were brought about more speedily, and death followed more quickly, in proportion to the youth of the patient.

The inferences deducible from the results of these several cases, relative to treatment, point to the practical precept of early extirpation; we have evidence of its beneficial results in case No. 4, though ulceration, with its characteristic attendant symptoms, had just manifested themselves; the part was excised, the wound healed, and there has been no return of the disease, though a period of over three years has now elapsed.

In cases Nos. 2, 3, 5, and 6, the operation, I conceive, was had recourse to after the lymphatics and capillaries were charged with the product of the cancerous alteration; and though in some instances the wounds readily healed, yet in a short time the secondary results, the effects of the absorption, manifested themselves in the form of encephaloid disease. So firmly convinced am I of the line of treatment to be adopted in these cases, that I would advise all warts, when situated on the face or elsewhere, to be removed by the knife as early as possible, no matter how youthful the patient may be, as they all have a tendency in advancing years to degenerate in the manner which I have endeavored to represent and elucidate.

Dr. Beatty thought that the cases brought under the noticeof the Society that evening by Mr. Butcher, were both instructive and interesting in the highest degree, and said he fully concurred in his concluding observation, which shoud be borne in mind by every person who was likely to encounter those remarkable affections—namely, that a suspicious looking wart, especially on the face, should no be tampered with by attempting to effect a cure, but should be extirpated with the least possible delay. The remarks of Mr. Butcher had brought to his recollection the case of an acquaintance of his own, a very handsome young lady, who had upon her right cheek a wart of a very suspicious nature. Meeting her in the street one day, he asked her about it, and she informed him that it was a thing of no consequence whatever, and that her apothecary was daily in the habit of applying caustic to the wart with a view of curing it. He advised her to go to some eminent surgeon and have it removed; she took his advice; the wart was extirpated, and from that day to the present there was no return of the disease. But if she had permitted more time to pass without the removal of the excrescence, she would in all probability be now in the condition of one of the unfortunate patients described by Mr. Butcher in his interesting communication.

Dr. Jacob believed the cases adduced by Mr. Butcher to be well worthy the consideration of the profession; and trusted that they would have the effect of calling attention to the subject, as it might perhaps lead to more dis-

tinct arrangement of these affections, than had been hitherto made. Last year he (Dr. Jacob) exhibited to the Society a case in which he had extirpated a tumor of a malignant nature from the orbit, and as such a case must be watched with great care, of course he kept the patient within view. It was one of carcinomatous tumor of the orbit, with encephaloid growth engrafted upon it. The poor man had since come back to him with a return of the disease, a great development of encephaloid structure having taken place in the orbit and on the side of the head. The instructive question in those cases was, whether they ought or ought not to extirpate the disease. Eighteen months had gone by since the tumor was removed in the case to which he referred; and therefore he thought that the argument for the operation amounted to this, that the man's life was prolonged, though not saved by it.—*Dublin Medical Press.*

[The foregoing eminently practical paper, well deserves the attentive perusal of the Surgeon. Dr. Butcher has opened up a field for inquiry not hitherto cultivated, and we doubt not, our readers will feel as grateful for his investigations, as we have much pleasure in acknowledging that we are.]—*Eds. Can. Med. Journal.*

Relapsing Fever. By Professor Austin Flint.—On reviewing the results developed by the foregoing analysis, and comparing them with the distinctive traits of *Relapsing fever*, as presented in the account preliminary to the analysis, the correspondence is certainly striking. The access was abrupt in one-half the cases in which this point was ascertained, and unusually short in duration in the remainder. In 5-14 of the cases, there were no evidences of delirium; and in all the cases, this, together with other head symptoms, was slight.

In 10-15 of the cases there was entire absence of diarrhea, and in one case only was this symptom in any degree prominent or persistent. Vomiting was not a prominent feature being present at the access or commencement in but 8-13, and afterward in but three instances. Iliac tenderness was very slight in every case, and in six cases tenderness over the epigastrium, which is extremely rare in *Typhoid*

and *Typhus* fever, was marked. Meteorism was considerable in but a single instance, and absent in several cases. The abdominal symptoms, thus, were absent or slight in a remarkable degree, unless the cases were of the *Typhus* type, and it is to be borne in mind that none of the cases were considered to be of that type. The chest symptoms, and the circulation offer nothing worthy of particular note, except that the frequency of the pulse was considerably less than is stated to belong to the history of Relapsing fever. Sweating and moisture occurred in a very large proportion of the cases, in this respect presenting a contrast with the average occurrence of these symptoms in *Typus* and *Typhoid* fever; but differing from other observations with respect to *Relapsing* fever in the fact that these symptoms did not occur at the time of the temporary or permanent convalescence. Mild jaundice, a very rare event in *Typhus* or *Typhoid* fever, was present in *two* of the fifteen cases. But the most striking of all the points of correspondence relate, first, to eruptions, and, second, to the relapses. In none of the cases was there an eruption, a fact which would certainly be very remarkable with respect to the same number of cases of the *Typhus* or *Typhoid* type occurring successively. The circumstances pertaining to the relapses, viz., the duration of the first febrile career, of the interval, and of the second febrile attack, accord with the previous description of these events as they are stated to occur in *Relapsing fever*. Finally, the absence of a fatal tendency is exemplified by the fact that all the cases ended in recovery.

In view of these facts the conclusion seems unavoidable that the cases of fever characterized by relapses, among those which came under my observation in 1850–'51, presented the distinctive traits attributed to *Relapsing fever* sufficiently marked to entitle them to be ranked in the class of cases which have been described by different observers as a peculiar form of Continued Fever. This conclusion does not necessarily involve the position that the traits distinguishing the cases authorize their separation as cases of an affection entirely distinct from *Typhus* or *Typhoid* fever. It of course follows either that this position is correct, or that the *Typhoid* or *Typhus* forms of Continued Fever occasionally exhibit, as peculiar modifications, the symptoms which are considered the diag-

nostic features of *Relapsing fever* by those who regard the latter as a separate form of the disease. I am not prepared to discuss the relative merits of these inferences.—*Buffalo Med. Journal.*

Liberality of a Foreign Critic.—(From the British and Foreign and Medico-Chirurgical Review, for July, 1852.)—"Dr. Gross is quite correct in asserting that all the treatises on this subject as yet published in the English language, are mere outlines, which no one has attempted to render at all worthy companions to such works as those of Lawrence on 'Hernia,' Mackenzie on the 'Diseases of the Eye,' Budd on the 'Liver,' and Curling on the 'Testis.' It has remained for an American writer to wipe away this reproach; and so completely has the task been fulfilled, that we venture to predict for Dr. Gross's treatise (on the *urinary organs*) a permanent place in the literature of surgery, worthy to rank with the best works of the present age. Not merely is the matter good, but the getting-up of the volume is most creditable to Transatlantic enterprise; the paper and print would do credit to a first-rate London establishment; and the numerous wood-cuts which illustrate it, demonstrate that America is making rapid advances in this department of art.

"We have already expressed our high appreciation of Professor Gross's treatise; and it gives us much regret to feel compelled to state that the comparison of the two is, on almost every point, disadvantageous to Mr. Coulson. We acknowledge that we have tried his work by a high standard; but we think that we are fully justified in so doing, considering how long its author has been before the public as a surgical writer, and how high a position he evidently desires to attain. There is scarcely any point on which his work is not inferior to Dr. Gross's; and we have been continually led to feel, in our examination of it, how seriously defective it is, alike in method and in completeness; so that its perusal has left us with the conviction, that scarcely any one subject has been satisfactorily treated, scarcely any point of pathology or practice fairly and fully disposed of. We can assure Mr. Coulson that we state this with regret, and with no other moive than that which arises from our desire to discharge

our critical duty faithfully and impartially. That our verdict will be confirmed by any competent judge, who may take the same pains that we have done in the comparison of the two works before us, we have not the smallest doubt; and we have even a sufficiently good opinion of Mr. Coulson's own candor, to believe, that if he will examine Professor Gross's treatise for himself, he will admit our case to be so strong, that he will not feel it necessary to impute to us any bias to his disadvantage."

On the Reciprocal Influence of Acute Diseases and Menstruation. By M. Herard.—M. Herard terminates a recent memoir with the following conclusions:—1. All acute diseases exert a pretty similar effect on menstruation.—2. This influence varies accordingly as the diseases become developed during a menstrual epoch, or during an interval.—3. In the first of these cases the menses are usually suppressed completely or incompletely, when they may reappear after some hours or days, though usually in diminished quantity. The patients regard the suppression as being the cause of the febrile disease, although the contrary is the fact: and even in the case of acute febrile disease becoming manifested after suppression, we must regard it as a consequence of the chill that has produced this.—4. When an acute febrile disease is developed in the interval, if the next epoch is near at hand, so that the fever continnes to it, the menstruation is favored by the increased hæmorrhagic congestion of the uterus and ovaries.—5. The menses are usually absent or notably diminished in quantity, at the periods which occur during the decline of a disease, or in convalescence. This secondary amenorrhœa, though sometimes persistent, usually only continues for from one to three months.—6. The menstrual eruption in nowise predisposes to disease.—7. Menstruation exerts no appreciable influence on the issue of acute febrile affections. The progress and termination of these are the same, whether the discharge appears or not, whether it is increased or diminished in quantity, is earlier or later in appearance, or whether this takes place at the beginning or end of the affection.—8. In treating acute febrile affections, it is the

condition of the disease that must engage our attention; for it is rare that any special therapeutical indication is derivable from the state of the menses; and we must act absolutely in the same way if the menses are on the point of appearing, or are expected, as if they were not so.—9. Bloodletting does not, in general, prevent their appearance or continuance—10. The sudden suppression of the menses by the development of an acute febrile disease, or amenorrhœa consecutive to such disease, does not, in general, call for any special treatment.—*L'Union Medicale*, 1851, No. 149.

Muscular Power of the Insane. By M. Morel.—A general popular error prevails that the insane are endowed with inordinate muscular power; and this explains why so many persons are brought to the Mareville Asylum fearfully tied and corded. When M. Morel was first appointed to this, he found numerous patients bound up, reputed dangerous, and especially so because of their vociferations. He set them at liberty, without any ill-effect, and attributes much of the violence that had previously occurred to the ill-conduct of the attendants. He agrees with Jacobi, that, as a general rule, the insane exhibit no inordinate muscular power; and some of the patients of almost colossal stature are easily managed by one person. Indeed, the insane, when engaged in manual labor, soon tire, and require frequent repose. If some of them, by exception, work with a feverish activity, and display great strength, the majority are dejected and languid. The persons in whom he has met with the greatest development of muscular power, belong the following ctaegories: —1. Persons of small stature, delicate complexion, and nervous temperament; and especially females who appear exhausted by their cries and agitation. Among such miserable-looking beings, a power of resistance is developed under certain circumstances, which defies the united energies of several attendants; 2. Insane epileptics; 3. Monimaniacs, who are not yet exhausted by the disease or irrational treatment. When their passion is opposed, these persons sometimes manifest a resistance only to be overcome by several attendants.—*Annal. Med. Psych.*

On the General Improvement in the Condition of the Insane. By M. Morel.—M. Morel observes, that since the reform of the French asylums commenced by Pinel and Esquirol, and continued by Ferrus, the physiological condition of their inhabitants has become quite changed; and remarks on their condition, which would have been just, twenty or thirty years since, are so no longer. Among the eight hundred patients of Mareville, one may seek in vain for the frightful types of degradation seen in different asylums but ten or fifteen years ago. The insane have a more civilized appearance, and are cleaner. The number of dirty patients is considerably diminished; and excrement-eaters are now only found quite exceptionally. For the purpose of clinical instruction, all the idiots and imbeciles of the asylum have been brought together, so that their special characteristics might be studied; and the graphic picture of their condition drawn by Esquirol is not now found of general application. Their condition has become ameliorated by the agency of work, exercise, gymnastics, good diet, and especially by their being transferred to healthy, well-ventilated, and well-lighted localities; for they are no longer confined within walls which prevent their view of the horizon. We must never deceive ourselves by the supposition that patients apparently in the most degraded state are insensible to the charms of nature. Surrounded by these, their features expand, and their physiognomy loses somewhat of its stupid appearance.—*Ibid.*

Obstinate Intermittent Coryza—Instantaneous Cure. By Dr. A. Menudier.—A lady, aged twenty-four, had been for some years subject to severe attacks of coryza, coming on twice or three times in a week, and lasting from twelve to thirty-six hours. On one occasion, she was anxious to have the disease arrested immediately. M. Menudier thereupon applied a sinapism from the scapulæ to the loins; in a quarter of an hour, the coryza began to diminish, and in three quarters of an hour the patient could no longer bear the mustard. The skin was reddened; but the coryza was cured; and had not returned at the end of three months.—*London Journ. Med.*

American Institute of Homœopathy.—The proceedings of this body at their session in Baltimore, have been published. The whole number of members, as appears by their catalogue, which includes both the living and the dead, is 271. This aggregate is made up from the whole country, from Canada to California, and including both with the several States of the Union. As all the tribe in New York, of whom we have any knowledge, even the practitioners of Dentistry, fraternizing with them, are found in the list, we suppose that it is safe to infer that this sum total of 271 is the extent of "Homœopathic practitioners," who can be counted up on this continent.

As confirmatory of the estimate of the statistics of the sect, a few other data are found in this document which we subjoin.

In the city and county of Philadelphia, there are said to be *sixty* Homœopathic practitioners, and it is claimed that these are patronized by 90,000! one fifth of the population, which is rated at 450,000. A very modest pretension, certainly.

From Massachusetts, it appears that there are *fifty-nine* in the commonwealth, who, we are told, are all doing "a *paying* business" in Homœopathy; which is certainly high praise, for the Yankees are proverbial for "minding the main chance," and it is not unprofessional in the craft everywhere to imitate them.

The only other statistical items which can be gleaned from this document, are that, incidentally, mention is made of "4 Homœopaths in New Hampshire, 3 in Maine, 4 in Rhode Island, and 3 in other States," meaning New England, we suppose. But for greater accuracy, and for the reason that we would rather overrate than underrate the forces of the tribe, we refer to the general catalogue, by which it appears that the ratio for the different States stands thus, viz:—

New York, 83; Pennsylvania, 47; Massachusetts, 32; Ohio, 19; New Jersey, 14; Maine, 12; Connecticut, 10; Rhode Island, 10; New Hampshire, 7; Maryland, 7; California, 4; Louisiana, 3; Texas, 2; District of Columbia, 2; Alabama, 2; Virginia, 2; Illinois, 2; Wisconsin, Georgia, Delaware, and Canada, each 1; and a few others not located. Such is the classification of all the members of this National Society, which no doubt

records the names of all in the country whom they recognize, good, bad, and indifferent, for many of these are certainly indifferent enough. If a similar enumeration of Physicians were made, there would be found five times as many in New York city alone, as there are Homœopaths in the whole country.

Nothing demonstrates the insignificance of the sect more clearly than these statistics furnished by themselves. And if they could only be required to show their colors in every place, by announcing themselves as Homœopaths on their signs and cards, as honesty demands, the public could then discriminate between them and physicians, and the line of distinction being thus drawn, they would be impotent, even though as numerous as the locusts of Egypt.—*N. Y. Med. Gaz.*

Improved Syringe.—Allusion was made, some months since, to an essential improvement in that useful instrument, the syringe. A Boston physician has brought it to a surprising degree of perfection. In workmanship the instruments are unrivalled; but their real value consists in the applicability of the barrel and flexible tubes to various purposes in maintaining health when mechanical assistance is necessary; and in fact to every purpose for which a syringe is needed. Both hands of the individual are not required, in operating upon himself, which is a decided improvement. We have not been accustomed to such highly finished articles, in this line, and the country has reason to be proud of them. It is quite needless to particularise their exact construction, to show the superiority of the invention over the common syringes of the shops. Those desirous of an examination are invited to call for that purpose. Facilities for manufacturing are on an extensive scale, such as will at once meet the demand which the utility, compactness, beauty, and economy of the article will create.—*Boston Med. and Surg. Jour.*

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UNIVERSITY OF LOUISVILLE.

THE preliminary course of lectures in the medical department of the University of Louisville commences on Monday next. We take this occasion to say that Prof. Flint and Prof. Palmer are expected during the second week of the course, and that they will continue their lectures in the University throughout the winter. This statement is rendered necessary in consequence of a report which prevails in some parts of the country, that they were to remain in Louisville only a part of the winter. After the course is over here, Prof. Flint will deliver the course on Theory and Practice in the University of Buffalo.

KENTUCKY MEDICAL SOCIETY.

OUR Kentucky brethren, we trust, will not forget the meeting of the State Medical Society, which is to take place in this city on the 20th instant. We owe it to ourselves to foster and sustain this society. In several States younger than Kentucky, similar societies are in successful operation, conferring benefits upon the profession, and honor upon their

members. We predict that the papers and reports read at the approaching meeting, will be of a character to reflect credit upon the profession in Kentucky. Association, harmony, and concert of action, this is the secret of success. We shall advance medicine in proportion as we unite our efforts to the common end—as we meet together and communicate knowledge, correct errors, start inquiries, encourage investigation, reward discoveries, and mutually fire and stimulate the minds of one another. The London Philosophical Society, the British Association, the Institute of France—how much does the world owe to these societies; and the American Associations, Medical and Scientific, young as they are, how much have they already done to advance the science of our country! Let every physician feel that he has an interest in, and owes a duty to the Medical Society of his State.

DR. SAMUEL A. CARTWRIGHT.

Dr. Cartwright is one of the most eminent physicians of the South. A great many years ago he published, in the Medical Recorder, papers on Bilious Fever, which won for him a national reputation. He is an independent, original thinker, a bold, clear, vigorous writer, and a practitioner of remarkable solidity of judgment. He is greatly esteemed for the excellence of his moral character, his probity, simplicity, and amiability in private life, not less than for his professional learning and skill. We have been much interested in some controversies in which he has lately become involved, by his advocacy of certain novel views in anatomy and physiology. First, he performed an operation upon a female laboring under disease of the ovarium, in which he believed he had succeeded in passing a catheter along one of the Fallopian tubes into that organ. Dr. Brickell questioned the reality of the performance, and this has brought on an angry discussion. We are sorry that the discussion has assumed this character. There can be no question that both gentlemen are conscientious. Dr. Cartwright seriously believes that he did catheterize the Fallopian tube; there is no doubt in the world of that. Dr. Brickell does not believe that such an operation is practicable, but in expressing a doubt on this point it was not his intention to call in question the veracity of Dr. Cartwright. It is a matter about which doubts are very naturally entertained; but Dr. Cartwright may safely leave his reputation for veracity to take care of itself. No one

will doubt that if he made a mistake about this operation, it was an honest mistake.

Our friend has warmly embraced Mrs. Willard's theory of the circulation of the blood, and this has involved him in a controversy with another party. He displays great tact, gallantry, spirit, and address, in his support of the hypothesis of this "*Filia Nata Jovis*," as he styles the fair authoress, and though he may have the worst of the argument, he renews the contest with an alacrity worthy of a veteran as he is. One cannot but admire the courage with which he returns to the contest after he had seemed to have been overwhelmed by his adversary. But, we must say, there appears to us but small hope for the doctor and his "*Hematokinety*," with such antagonists as Dr. Ely, Dr. Riddell, and above all, Dr. Dowler, "the alligator king." This discussion, however, will go on, as all scientific discussions should, good-naturedly. The readers of the New Orleans medical Journal have had a treat in the articles which it has called forth. Dr. Cartwright's last is full of humor and vivacity.

Last, but not least, Dr. Cartwright has "a philosophy of the negro constitution," which has called out a swarm of critics. He is a true believer in the inferiority of the negro race. He holds to slavery as a Bible doctrine. He insists that the Africans, as the sons of Ham, are doomed to bondage, and that it is a violation of the highest law to attempt to set them free. They are inferior, he maintains, not only from habit and moral influences, but from a law of organization which can neither be reversed nor amended. The sum of his doctrine is that they have less mind because they consume less oxygen; that they are not only not capable of breathing as much air, but do not incline to breathe as pure air, and hence, when they sleep, "cover their heads with a blanket." These views—this "philosophy of the negro constitution"—he defends stoutly and resolutely against assailants of whatever creed, and from whatever quarter. He is in earnest. He believes every word of the doctrine, and in defence of it, evinces no common ingenuity and research, and most uncommon resolution. Numbers do not intimidate him, nor is he awed by authorities however high or venerable. We apprehend, however, that his philosophy is not likely to gain much favor. It is assailed in the very quarter where it seemed most probable that it would meet with advocates—in the extreme South—and with a force of argument and fact which our learned friend will find it difficult to resist.

We like Dr. Cartwright's independence, and his industry is to be commended. We are sure that we express the feelings of all his brethren,

not excepting his critics, when we wish that he may live yet as many years as he has already spent in honorable professional labor.

NEW MEDICAL SCHOOLS.

We owe an apology to our friends of the new Medical School of Cincinnati for not noticing their organization in our last number. It is styled the "*Miami Medical College of Cincinnati,*" and Dr. Mussey, late of the Medical College of Ohio, is at the head of it. It would appear from this that the venerable and distinguished surgeon was not so weary of teaching as some of his friends supposed he had grown. His colleagues are Drs. Judkins, Avery, Davis, White, Mendenhall, Murphy, Comegys, and John Locke, Jr. We heartily wish them success in their enterprise, assured as we are that their institution will be conducted upon principles honorable to themselves and to the profession.

And we have to record the incorporation of still another medical college—the *Savannah Medical Institute.* This is the second medical school for Georgia. The gentlemen composing the faculty are Drs. Arnold, Kollock, Bulloch, Byrd, Howard, Martin, West, and Read; "young, ardent, energetic" men, we learn from the Charleston Medical Journal, of whom the editor remarks that, "with a reputation to earn, there is no reason to fear that they will prove to be laggards in the race for professorial as well as professional distinction."

PROFESSIONAL APPOINTMENTS.

Dr. Robert E. Rogers, of the University of Virginia, has been elected to the Chair of Chemistry in the University of Pennsylvania, made vacant by the death of his brother, Dr. James B. Rogers, and is succeeded by Dr. J. Lawrence Smith, of the University of Louisiana. We congratulate the University of Virginia on having secured the services of a gentleman every way so eminently fitted for the station as Dr. Smith.

Dr. G. M. Moore, of Woodstock, Vt., takes the Chair of Anatomy in the University of Buffalo, vacated by Dr. Palmer. In the Pennsylvania Medical College, Drs. F. G. Smith, J. M. Allen, and J. J. Reese, have received appointments, and it is believed that the institution has been much strengthened by their accession to its Faculty.

POISONOUS CHLOROFORM.

From the subjoined article, which we copy from the *Boston Medical and Surgical Journal* of the 8th of September, it will be seen that Dr. Jackson thinks he has discovered the cause of the many accidents which have resulted from the inhalation of Chloroform. Every philanthropist will rejoice if the conjecture of Dr. Jackson should prove to be well-founded. It cannot be disguised that there is danger in the use of this agent as commonly administered. The numerous deaths that have followed its administration have deterred many physicians from its use altogether, while they have rendered all apprehensive of its consequences. He will be a true benefactor of his race who shall disarm this invaluable remedy of its terrors. Dr. Jackson's communication is as follows:

The numerous deaths which have recently taken place from the inhalation of chloroform, seem to require that I should state what I know upon this subject, without waiting for more extended researches which I have now in progress; for a word in time may save human life, and I shall therefore present my views, even though some may think that I ought to wait until my work is completed to its full extent before publication. I have formerly been charged with dilatoriness in presenting my discoveries to the public, and wish to avoid a repetition of this accusation, even though my work, in its present state, is not so complete as would be required for scientific purposes.

I have long had a strong suspicion that the very sudden deaths resulting from the inhalation of chloroform, must have been produced by the presence of some poisonous compound of amyle, the hypothetical radical of Fusel oil, or the oil of whisky; and I began a series of researches upon this subject several years ago, but was called off from my work by unexpected persecutions. This work I have resumed, and I will now state what facts and inductions I am able to lay before the public:

1st. When chloroform, and the alcoholic solution of it called chloric ether, was made from *pure* alcohol diluted with water, no fatal accidents took place from its judicious administration.

2d. When chloroform was made, as it now frequently is, from common corn, rye, and potato whisky, deaths began to occur, even when the utmost care was taken in its administration.

3d. In the Chelsea case, where this kind of chloroform was probably contained in the alcoholic solution incorrectly called chloric ether, death took place in a very sudden manner, and the post-mortem appearances of the subject indicated the usual effects of poisoning by chloroform.

From these data, it might justly be inferred that some poisonous matter exists in the cheap chloroform of commerce, and I suspected that it arose from the Fusel oil which exists in whisky. This opinion, at my suggestion, was published by two of my friends, to put the public on their guard, and those gentlemen urgently advised that physicians and surgeons should return to the use of pure sulphuric ether, (oxide of ethyle,) as originally prescribed by me.

It is well known that I have always preferred my original anæsthetic agent to all the substitutes that have been proposed since; but still I have always been willing to give the proposed substitutes a fair trial, and did try them all, first upon myself, and then upon such of my pupils as felt willing to allow the experiment to be made upon them. I also, in a measure, compromised with that powerful anæsthetic agent chloroform, by mixing small proportions of it, about one-fourth or fifth part, with sulphuric ether, so as to concentrate the anæsthetic agent into a smaller bulk, and I have extensively used this preparation in the production of anæsthesia, and without producing any dangerous or even unpleasant symptoms in any case, but I always took care to ascertain that the chloroform used by me was pure.

Having, during the last month, succeeded in procuring some very pure Fusel oil, (of whisky,) I undertook the researches which have resulted in the conviction that it is this amyle compound that produces the poisonous matter of certain kinds of chloroform. When mixed with hyperchlorite of lime (bleaching powder) and water, in the same way as we prepare alcohol for the production and distillation of chloroform, I found that the mixture in the retort, after agitation and standing some time, became warm, indicating that a reaction was taking place between the Fusel oil and the hyperchlorite of lime.

After some hours the retort was placed in a water-bath, and distillation was effected, the volatilized liquid being condensed by one of Liebig's condensers. A clear colorless liquid came over, which was at once recognized as having the peculiar *odor of bad chloroform.* It is, perhaps, a *ter chloride of amyle,* but has not yet been submitted to analysis. It is so powerful that merely smelling of it makes one dizzy, and working over it made me so sick that I was obliged to go out of doors for fresh air several times during my operations on it. In order to make sure that the Fusel oil was all decomposed, I again mixed the product of the distillation above mentioned with a new lot of bleaching powder, and water; and after three hours, with frequent agitation, it was again distilled, and gave what I regard as the pure unmixed poison. This I am now to test on such animals as have proved good ether subjects, and shall make report of my results in this Journal.

If my views are correct, it follows:

1st. That all chloroform intended for inhalation as an anæsthetic agent should be prepared from pure rectified alcohol, to be diluted with water when used for distillation, from hyperchlorite of lime.

2d. That no druggist should sell for anæsthetic uses any chloroform which is not known to have been properly prepared as above suggested.

3d. That the mixture of chloroform and alcohol, commercially known under the name of strong chloric ether, must be made with the same precautions as chloroform.

There is less danger of the existence of Fusel oil in sulphuric ether, which is always made of strong rectified alcohol.

There is more danger of the existence of sulphurous acid in this liquid,

and that is a dangerous poison, but it is one readily detected; and persons will object to inhaling ether containing it, on account of its well-known disagreeable odor of burning sulphur.

TRANSYLVANIA MEDICAL JOURNAL.

This journal has been enlarged and improved in its appearance, and is now under the management of Dr. L. J. Frazee, formerly of Maysville, Ky. Dr. Frazee is known to the profession by a small volume which he published on his return from Europe, a few years ago, entitled "The Medical Student in Paris." He is a pleasant writer, a gentleman of attainments, ambitious of eminence in his profession, and, we have no doubt, will edit the Transylvania Journal well. He has our best wishes, and we can venture to promise him that if his new field of labor should not prove profitable in a pecuniary way, it will give him fine scope for intellectual exercise.

DYSENTERY AND CHOLERA.

A sporadic case of dysentery is still occasionally seen in this city, but the disease has almost disappeared. In conversing with our medical friends, we find that the opiate practice is that which has given most satisfaction in this complaint during the past season. The opiate has been variously combined, as with sulphate of magnesia, turpentine, calomel, and astringents; but, in the practice of all, an opiate has entered into the treatment. Opium in some of its forms, has unquestionably been the efficient agent. In our judgment, next to opium, the saline purgatives have proved most beneficial. We have not seen calomel used with advantage in any case.

Cholera seems to linger in some parts of the country, but Louisville has happily remained exempt from the pestilence. During the last days of September, it was reported to be prevalent in Henderson, Ky., and to have carried off some of the best citizens of that place. The disease is now rarely heard of on boats plying the Mississippi or Ohio.

OBITUARY.

Of the many promising and eminent physicians of Philadelphia, whose premature deaths the profession has been called to mourn within the last few months, no one will be more generally or deeply lamented than Dr. Isaac Parrish, whose obituary we find in several of our exchanges. He died of dysentery, a short time since, after a few days' illness. The *Medical Examiner* truly remarks:

"Few men could be less spared than Dr. Parrish; he was essentially a working man in the profession, both in the daily routine of practical duties, and in everything calculated to advance and elevate it. To the pages of this journal, as well as of many of the other medical and daily papers, he was a constant and welcome contributor, and his articles were always characterized by sound judgment and practical bearing. He died in the prime of life, and in the full tide of a successful practice, beloved and respected by all who knew him."

Dr. Parrish impressed the stranger at once by a manner singularly frank and sincere, and the phrase "the just upright man," is one that now rises to our mind as descriptive of his character. In him we feel that our profession has lost an ornament.

MEDICAL FEUDS.

The Boston Medical and Surgical Journal refers thus to a quarrel now in progress between the Homœopathists and the Hydropathists:

By an article on hydropathy, in the Philadelphia Journal of Homœopathy, it seems that the infinitesimal gentlemen have no confidence in the professors of the water system of medication. They express a proper degree of horror for anything so shockingly unscientific as water, as a remedial agent. Now both schools utterly condemn and abominate the regular practice; and it is a little singular they should thus quarrel among themselves.

THE

WESTERN JOURNAL

OF

MEDICINE AND SURGERY.

NOVEMBER, 1852.

Art. I—*Report on Asiatic Cholera, as it appeared in New Albany Indiana, in the year* 1852. By Albert de Leszczynski, M. D. of New Albany.

The steamer Midas, from New Orleans for Cincinnati, landed at our port in the night of the 5th of June, having on board, besides her cabin passengers, one hundred and seventy-five deck passengers. The Midas is one of the smaller boats. She had been on her voyage, up to her landing here, about fourteen days. During this time several of her deck passengers had sickened, and sixteen of them, after a short illness, died. On the 6th of June, at 11 o'clock, A. M., I was called upon by a passenger of this boat to see his infant child, which was then in a dying condition. It was attacked by hydrocephalus acutus. Meanwhile, several of our citizens, having heard a report that the cholera had broken out on the Midas, and that

several persons had died of it, as also the very deplorable condition of the yet remaining passengers, went on board to assist the needy, and waited upon me to see and assist the afflicted. I went and found the report but too true. The Asiatic Cholera was on board. Two children, a boy and a girl of respectively 11 and 8 years, were dying. Four others, three females and a man, were hurrying towards collapse, and a number of others were suffering with that malign diarrhea usually enumerated among the "premonitory symptoms."

As I afterwards learned, some of our Germano—American citizens—following the dictates of a kind heart, had induced a number of the passengers of the Midas to land and stay here, to avoid further exposure. Four moved into a private family, and twenty-eight of them into a two-story frame house, in the suburbs of our city.

On the 6th of June, at 10 o'clock, P. M., I was called to see the first patients of these immigrants, (of which it was my lot to treat professionally twenty-eight.) One of them was a man, the other a female. Both were attacked with cholera, and both were in collapse. What I understand by cholera, I shall describe under the head of *symptoms*, and only add that the house which had been taken possession of by the immigrants became, in a short lapse of time, the fruitful centre of contagion, whence the epidemic spread in various directions, slaying with unerring hand, all those who did not apply for timely aid. The entire number of cholera patients treated by me, up to the present time, is forty-nine.

Symptoms.—I shall here recite all symptoms. To give it a greater order, I shall divide the disease into three stages, (as indeed nature self-divides it,) and delineate the marked symptoms of each division.

Cholera Asiatica observes:

1. The stadium prodromorum—the stage of the forerunners, or that of the premonitory symptoms.

2. Stadium algidum, seu cyanoticum—that of the collapse, and

3. The stadium reactionis—the stage of reaction.

1. Stadium Prodromorum. *Symptoms.*—In some of the epidemics which, at different epochs, have visited large divisions of the earth, premonitory symptoms have been wanting to a great degree; in others they were of longer or shorter duration. *Here,* I observed, objectively and subjectively, the following: Great anxiety; pressure in the præcordial region; vertigo to a greater or lesser extent; slight, or sometimes stronger, headache; ringing in the ears; frequent sighing; dyspnoëa. The appetite is disturbed; the energy depressed; a feeling of weakness, mostly in the lower extremities; and a sensation of a peculiar lightness in the superior posterior part of the thorax. If the patient felt free of headache, the head felt remarkably clear, and the perceptive faculties seemed rather increased. The pulse was quick and easily compressible; the urine became reddish and scanty; the skin, in some instances dry, in others secreted, profusely, a clammy, even cold perspiration. Lastly appeared borborygmi, resembling the moving of gases and fluids, and finally diarrhea. It must not be supposed that all these symptoms took place gradually. Between the first and diarrhea, in some instances, hardly a few hours intervened. Sometimes diarrhea appeared without the slightest admonition. It generally lasted from three to six days but in some cases, hardly as many hours, before changing into the next stage. The diarrhea was not as painful as that accompanying dysentery or typhus; on the contrary, it always, as it were, relieved the patient, who, when it lasted any time, gradually became exhausted. The color of the stools in the commencement, generally, was of a dark green, sometimes light yellow, gradually changing into a lighter one; the quantity copious; the evacuated matter fœtid, but becoming less and less so, as the dis-

ease advanced, and when those flocky bodies were in a gradually increasing quantity admixed, which appear in the so-named rice-water discharges. The thirst was great; the tongue white, broad and pale—sometimes livid; the eyes sunk more or less, sometimes surrounded by a dark livid areola; the conjunctiva bulbi, in this stage, remarkably bloodless. Here I must mention the remarkable fact that all, previous to diarrhea, labored under constipation.

2. Stadium Toxicum, Algidum seu cyanoticum.—Among those forty-nine patients treated by me, there was not one where the cyanotic stage had appeared without, although sometimes only a very short, preceding ailment. Of the entire number, twelve cases were collapsed, twenty-six seriously affected, threatening collapsus, and eleven less dangerous. Ten of the collapsed cases complained for several days previous, (and previous to my attendance.) One was collapsed three, another seven hours after the appearance of the first symptoms.

Symptoms.—Vomiting and purging of rice-water-like, scentless matters, which continue incessantly, until at last exhausted nature ceases to evacuate. The eyes are sunk into the orbita, the conjuctiva bulbi et palpebrarum is highly injected, the rami of the blood-vessels form a net-work over the eye-ball. The head is hot, covered with a clammy perspiration; the tongue, cheeks, ears, are cold as those of a corpse; the expirations are likewise very cold; respiration very hurried; the pulse, at the commencement of this stage, small and fluttering; soon ceases to beat at the extremities—and even that of the carotids can shortly not be felt. The contractions of the heart are very feeble and intermitting; the respiratory murmur scarcely audible. The thorax is still warm; there is great pain in the epigastrium, like the burning of a coal. The patient complains of violent cramps, which in most cases commenced in the great toe, moving up the legs into the abdomen, when dysponëa takes place. The

skin, having lost its turgor, in most cases is covered with cold, clammy perspiration, and corrugated on the extremities and neck, but strongest on the fingers. The thirst is extreme; the patient continually calls for cold water, which, as soon as taken, is emitted. The papillæ of the tongue are shrunk, that organ self-covered with a slimy, ropy mucus, of the same repugnant smell as that of the slime evacuated by an emetic. The secretion of saliva decreases, and the patients complain of burning in the throat, which partially causes their insatiable thirst. The urinary secretion is entirely suppressed. The patients retained their mental faculties to the last, although some of them, in some cases, were impaired so much, that they seemed to lay in perfect apathy, out of which they could only be aroused by shaking, or very loud talking. This stage lasted with those patients, I observed, from eight to forty-eight hours.

3. Stadium Reactionis.—Although a reaction takes place in the first stage, I shall pass it, and only describe the reaction I observed after collapsus. Only three of my patients died during the latter; in nine, reaction took place, but only three recovered entirely.

Symptoms.—The patients became easier; the radial pulse returned; evacuations ceased either entirely, or became less numerous, and strong congestions took place towards head and chest; the former, during advance of reaction, became hot, the sunken eyes elevated, the face alternately flushed and pale. Perspiration appeared, but the extremities remained cold with those who died, and became warm with those recovering. All lay in a comatose condition; the eyes were still much injected; the tongue, when protruded, broad, thick, dry, and trembling. Urine was voided, its color reddish brown, but became soon limpid with those who survived, containing a surplus of urea, (as uric acid) The gums and lips were covered

with sordes similar to that in typhus. This period lasted from two to six days, and with one, who recovered, above it.

THERAPEUTICS.—Various were the remedies I employed. The strongest excitantia served in some cases, in others, cerebro-spinantia and astringentia proved themselves of value. In the first stage, plumb. aceticum cum opio; or, if an idiosyncrasy against the latter existed, or as a precaution to children, I gave in its stead hyoscyamus. This, rest, avoidance of cold water and solid food, only permitting rice or barley-water, in very small quantities, soon restored the patients, and the primary changed into a secondary disease only in a few cases. But in every case where this occurred the patient was at fault. During collapse, the strongest stimulants were given. Camphor and aq. ammon. causticæ, diluted with wine, saved one case. I gave equal parts of aq. ammon. and tinct. camphor. ten drops half-hourly, in one ounce of Rhenish wine, and good chicken broth hourly. But, above all, I value the exhibition of Chloroform, from seventy to one hundred drops, in milk or mucilage, repeated every quarter or half hour, until vomiting ceases, or sleep supervenes. This remedy I used only in the latter part of our epidemic, after I had tested its unsurpassed efficacy in a violent case of colica pictonum, which strongly threatened to combine with gastritis, yielding neither to bleeding, opium, alum, croton oil, tobacco, nor cold-water injections—was subdued in a few hours after the subsequent internal exhibition of chloroform.

After the first effects of this substance: cessation of vomiting, of cramps, sleep, and, in some instances, perspiration—opium, gr. ¼–½, with plumb. acetic. grs. ii–iii, and pulv. camphor, gr. ½–i, were of excellent service. These latter remedies I gave until the entire suppression of the alvine evacuations, repeating it every two hours; after which I prescribed quinine sulph., grs. ii–iii, cum

opio, gr. ¼–½ pro dosis, repeating it every four or five hours. During the stadium of narcotization—vulgo the typhoid—I gave wine and the mineral acids, of which, particularly acid. sulphur. and alum were of the greatest import.

There were twenty-six cases threatening to run into collapse; chloroform invariably arrested their fatal progress. It proved of the greatest service, when even mosch. oriental. arnica, valerian, and ether, had been hopelessly employed. In all cases of the first and second stage, where the extremities were cold, applications of hot bricks to the feet, bottles filled with hot water surrounding the body, and sinapisms from the chest downwards, were very serviceable. In some cases I employed frictions and fomentations, with hot liquor, as recommended, the blister and the endermic exhibition of morphine on the epigastrium, but with very little success. The use of the lancet was, in every case, even in the stadium reactionis, out of the question. Regarding the local abstraction of blood, I must mention that, although it seemed indicated in one case of reaction, where I applied four cups to the nape of the neck, it proved worse than useless. Emetics of ipecacuanha were, in the first stage, of *some* service. The employment of calomel, either pure or combined with opium, proved always injurious, as also that of mild cathartics in general, even as late as the second day of convalescence, which, in a few cases, even provoked relapse. All gastric symptoms generally subsided after the employment of quinine with opium, as before mentioned; and only when this had been given for perhaps three or four days without producing evacuation, a slight purgative, such as pulv. rhei, grs. viii–x cum hydrarg. mur. m., grs i–iii, could safely be given. All the remedies I have mentioned have received a full and fair trial. Only, those I observed proving in-

jurious, I discontinued immediately, always having before my eyes the wished for end: the saving of human life.

After having stated the remedies which, in my practice, proved *most* successful, we shall pass a small critical review on a few of them, as regards their physiological effects, in order to judge of their respective merits, and the justness of their application.

Chloroform, Plumbi Acetas.—Of the other remedies mentioned I shall not treat, as they, at various times, have been fully discussed. Neither mean I to intimate that the combination of acetate of lead and opium has been only used by myself, as I am fully aware that many physicians employ it, and also that the great Dupuytren warmly advocated it.

Chloroform—Although its relation to the excitantia is undeniable, it belongs not solely under that head. There can be no doubt, from its external and internal application, that it more justly belongs to the cerebro-spinantia,—of which the narcotic acria also form an order. When applied externally, and undiluted, it causes pain, redness, and blistering, wherefore it must be an acrid. But as it causes disturbances of the cerebrum, as narcosis (anæsthesia) of the motory apparatus, (contractions and contortions of the muscles, especially of those of the face, particularly if given internally,) of the cerebellum and spinal marrow, (convulsions,) it follows that it is a cerebro-spinantium, and as such acts as the most powerful sedative ever known.

When inhaled, or exhibited internally, it at first causes the pulse to increase slightly in frequency, but, presently, as soon as narcotization commences, the circulation becomes slower, the pulse fuller and stronger. Here the stimulating powers cease, and the sedative effects commence. Its actions than extend from the cerebrum to the cerebellum, and elongated marrow to the spinal cord, and necessarily to the peripheric nerves. But, over the

great sympathetic nerve and its branches, it has not only no sedative, but a strong stimulating effect, as is thoroughly proved by the violent contractions of the uterus in labor, when this agent is given. Here labor rapidly progresses, although the parturient woman is entirely under its anæsthetic influence.

We shall now pass to review the physiological actions of the *stimulants*, and their changes in the system. I shall not give my individual opinion of processes they undergo and produce, but those of men of acknowledged authority, and add only, that close observation and experiment induce me to adhere to their doctrines.

All stimulants influence, more or less, the functions of the nervous centres,—the substance of the nerves. *Their local actions* are the more intense, the less their volatility and the stronger their affinity to the constituents of the organic structures and fluids. They act upon the mucous membranes and the external integuments, increasing in strength, the finer the epidermis covering them. The primary sensation will be that of heat—even intense burning—and only the most volatile of this class will cause a sensation of cold, a transient decrease of temperature, in consequence of their rapid evaporation. Parallel with the change of the sensible nerves of the dermis, appears an action upon the walls of the blood-vessels and their diameter. They become expanded, and hyperæmia, even congestion takes place in those parts acted upon by the stimulant. The blood vessels become narrower, the parts acted upon pale, anæmia takes place, which only, after a while, changes into hyperæmia. The local action of stimulants justifies the proposition, that there is a very slender difference between them and the acriä. Stimulants as æther, alcohol, and also chloroform, coagulate protein bodies.

Internally many of the stimulants cause contraction of the muscles of the intestines. Some of them act as

mild astringents, and frequently they increase the contractility of the biliary canals, the excretion of bile, and *it seems* as if they had the power to increase the secretion of this matter, by acting chemically upon the blood. All volatile stimulants are rapidly absorbed, and taken up into the circulation, without undergoing peculiar alterations, as other matters will; most so with alcohol, æther, volatile oils, and chloroform. In consequence of this, when taken into circulation they will very rapidly act upon the nervous centres, until acted upon by the oxygen, when they are excreted. Their excretion is nearly as rapid as their absorption. The lungs, kidneys, and perspiratory organs will return that portion of the stimulants which remained unaltered, as proved by the urine, perspiration and exhalation.

Lastly, all stimulants produce a change of function of the *nervous centres* and *circulation.* The former is announced by an exalted condition of the Psyche. Mental depression, sorrow, sleep, apathy, disappear, and give room to a joyful hilarity and contentment. The latter is observable by the stronger contractions of the heart, the fuller, vigorous pulse, and heightened color. Simultaneously we observe an increase of heat. The cause of this I should seek in the abnormally increased supply of carbon, which necessarily asks for an adequate supply of oxygen, which latter is admitted by *accelerated respiration.* The latter and rapid oxydation will elucidate the cause for accelerated circulation and calefaction. How much this, likewise, may influence the nervous centres, and how much their functions, I am not prepared to say.

The ultimate effect of stimulants is a physical and psychical depression. But that this should be only the consequence of the preceding excitement, I must doubt. Should this be so, then excitement would continue as long as the stimulant is retained within circulation. But this is not the case. Although excitement has long given away

to depression, the existence of stimuli within the body is proved by their exhalation per lungs. This is the case particularly with æther and alcoholic substances. There must be another cause for depression. According to my view, the blood has undergone a chemical change, a narcotization. This hypothesis is substantiated by the fact that *the fumes of liquor still are exhaled, although a person may have been intoxicated for more than twelve hours!* If all parts of the introduced stimulus had been expelled, how could the fumes still be observed? Here another fact strikes us: the purer the alcoholic substances used have been, the shorter will be narcotization, provided the quantity has been the same.

A short recapitulation and application of the just said, will elucidate the virtues of chloroform. It acts upon the elasticity of the coats of the blood-vessels and capillaries, on account of its energetic *antispasmodic* properties. It increases the mucous membranes, so much so, that even hyperæmia may be caused by it. It coagulates protein bodies, produces contractions of the muscles of the intestines, and promotes the excretion of bile, (as proved by vomiting after the exhibition of a large dose.) What kind of chemical changes it produces upon the blood, and if it really causes increased secretion of bile from it, must yet be decided. Its action upon the nervous centres are well enough known. It exhilerates, soothes and produces a temporary irritable and paralytical torpidity (anæsthesia) of the voluntary apparatus, but an increased action of the ganglionic nerves, and of the organs they enter.

It is best given in milk or mucilages. The dose is from 25 to 75 or 100 drops, repeated every quarter or half hour, as circumstances require. It even has been given with good success in colic to 200 drops pro dosis, and the dose repeated. This I state upon the authority of Dr. Clapp, Sen., of this city, which shows that the evil

effects of chloroform are not so great as some physicians maintain. But the exhibition of this, like every other article of the materia medica, is subjected to certain laws, indicated or contra-indicated. It is *indicated* in most spasmodic or neuralgic diseases or conditions of the system, where other remedies may have failed, or where an immediate effect is peremptorily called for. I, on my part, should give it in every case where a safe stimulant and antispasmodic is indicated, and only should find it

Contraindicated—1. In a *plethoric subject*, or inflammatory disease, as inflammatory colic, where, previous to its administration a vein is to be opened, when it safely may be given.—2. In a *cachectic subject*, or one laboring under a dyscrasia. In the first case the stimulating powers might be too great, causing apoplexy; in the second, the chemical action of the remedy might decompose the already diseased blood to such a degree that it becomes entirely unfit for nutrition, when death must ensue. We know the baneful influence of alcoholic substances on cachectic individuals. This acts in a similar manner. Although the modus operandi of the substance upon the blood is not perfectly understood, its *effects* we name narcotization.

Plumbi acetas.—All soluble preparations and oxydes of lead when brought into the stomach, are acted upon by the secretion of the latter, and intestines, and enter the circulation. They unite with the albumen, but not with the fibrine or hæmatosin of the blood, (Cozzi, 1844.) Lead, when once taken into circulation, acts particularly upon the secretory processes, the membranes and the spinal marrow, with its peripheric continuations and terminations. The motory and trophical nerves are most affected by it. The secretion of saliva, urine, fœcal matters, the perspiration, in short, most all, se.—and excretions are impaired, and only in rare cases we observe salivation, (previous to which, on the gums, the so named

"blue line.") In minute and not too long continued doses, lead will not cause important alterations of function of the nerves, but when continued for a very long period, or when given as oxyde, or salts, in an over dose (ʒss—ʒi or more), or if persons for a long time are exposed to its influence, then the functions of the nerves, ergo their substance is altered (as proved by post mort. sect.) In the stage of consecutive intoxication (as Tanquerel calls it), we observe hyperæsthesis, spasms of diverse peripheric portions of nerves, vertigo, headache, algia, even delirium and coma. This proves the affection of the ganglia, the spinal marrow and brain, the sources of the trophical, sensible, and motory nervous fibres.

When the nervous centres are acted upon by a powerful stimulant as Chloroform, when those alterations ascribed to it have been produced, it becomes necessary to check the profuse secretions, whilst simultaneously we must be thoughtful to stimulate the nervous system to continued action. This will be effected by plumbi acetas with opium. Both are cerebro-spinants, although differing materially in their individual effect. Opium given by itself I found insecure, not so the acetate of lead, which, when given alone, acts as a cerebro-spinal stimulant and astringent. As remarked before, where opium should be contra-indicated, or in children, hyoscyamus will fill its place. Of the other remedies used I shall not speak, as any materia medica will serve this purpose. I should not have spoken so lengthily had not justice to the remedies demanded it. Although perhaps I may be mistaken, I think it wrong in any author who only enumerates his remedies used in a certain disease, without showing his reasons for their administration.

In writing this essay I have not adopted the general rules. My motives in acting contrary to these were, to present to the reader first, the clinical, after which I shall pass to the theoretical part. To comply with this

view I will now give the results of (I am sorry to say) the only and rather imperfect *autopsy* I held. The subject of this examination was Henry Schmidt, a German immigrant, aged 39 or 40 years, of robust, plethoric constitution. Autopsy six hours after death.

Head and spine I have not examined, as I had neither the necessary instruments with me, nor time to do so.

Chest.—Trachea and bronchi slightly injected, of a sticky feel, as indeed were all membranes of the chest; the pleura injected, and the right heart filled with a black, tar-like blood, the former not crepitating when incised. The diaphragm injected.

Abdomen.—The liver soft, full of blood of the quality before stated. A *remarkable fact* must be mentioned here. Upon the convex side of the liver, about in the angle formed by two lines, one horizontal, drawn from the incisura interlobularis, continued five inches,—the second, vertical, drawn from the superior posterior margin, continued two and a half inches, I found a *gangrenous patch*, of the size of a ten cent piece; and beside it another of the size of a pea. Depth of gangrenescence, one inch. This corresponded to a pain in the right hypochondrium, complained of by this and other patients. But as I had no other chance of a post mortem examination, I do not know whether gangrene here was *accidental* or *essential*; my belief turns toward the latter conjecture.

The gall-bladder was full, containing ℥iv ℥v of dark green bile.

The stomach, enormously distended, contained about three quarts, of which two were rice-water-like fluids, containing the specific flocculi, and the third, meat and vegetable substances, which, eaten twenty-four hours previous, although excessive vomiting had taken place, never had been evacuated, nor acted upon by the gastric juice, so that it was easy to distinguish the species. The coats were excessively thin, the mucous membrane in

several places *injected.* It was easily scraped off with the handle of the scalpel. *The pancreas* was contracted, anæmic grayish-white, *the spleen* of normal texture and size, filled with tar-like blood; the smaller intestines distended with the same fluid and gas found in the stomach —in several places injected; the large intestines, distended with the same fluid, were otherwise normal. They contained a number of ascarides lumbricoides. I forgot to mention that some of the patients who died evacuated a great number of these worms. *The kidneys* were bloodless, the *bladder* contracted to the size of a large walnut.

ÆTIOLOGY.—Cholera Asiatica, as the name tells, a child of Asia, has been transplanted at various times from its place of birth to Europe, thence to our continent. Its genesis, although enshrouded much in darkness, has, by various observers, been attributed to *malaria.* Although *malaria* is a very vague idea, so much must be certain, that one, causing cholera, must be *sui generis.* Every plant bears its own seeds, and every specific seed generates its specific plant. When the lower division of the organic kingdom follows certain prescribed rules, why, I ask, should not the higher obey like ones? Certain causes produce certain chemical changes in the vegetable kingdom, according to irrevocable laws and specific causes must produce specific results in the animal kingdom, according to like irrevocable laws. Should the highest species of animal, man, only be an exception? As much as his growth, perfection, and decay are carried on by certain rules, so much must be his diseases. As soon as the normal laws conditioning health are infringed by a specific noxiousness entering the organism, so soon will another set of chemical actions be instituted, which, derogatory to growth, perfection, or preservation of the individual organism, will either cause a greater or lesser destruction of the individual fabric—*death or imperfect convalescence*—or the specific noxiousness has been consumed

in the process, or annihilated by art, when the primary chemical processes will be supreme again—health. The revelation of the ætiology of physiological and pathological processes, is the aim of the true physician.

If cholera is a specific disease—and no one doubts it—then it must necessarily have a specific cause,—it must be of a contagious nature. This latter has been denied. Every practitioner of the South knows that intermittens perniciosa—vulgo, congestive chills—appears at certain times, endemically and epidemically,—so that the question involuntarily arises in one's mind, can it be contagious? Of the law regulating the genesis of epidemics, endemics, and contagion, anon. Professor Fuchs, of Gœttingen, mentions in his manual of Nosology and Therapeutics, facts which strongly speak for the contagiousness of cholera. Some say it is not contagious, but infectious; some, again, it is portable. What is the difference between *contagium* and *infectio?* Infectio literally means, *the introduction of a mass—of an agent;* whilst *contagium* signifies, *an infection communicated by actual contact*—from *con* and *tango.* The signification of "portable," every one knows, and logic teaches, *when a portable matter can cause a specific disease,* IT MUST BE A CONTAGIUM. I hope that I shall prove that cholera, at least under certain circumstances, can be contagious.

As I mentioned in the introduction, four persons took lodgings in a private family, whilst the other twenty-eight moved into an empty house. H. S., of the first party, died. Over his body I held the post mortem examination. Two men, his host and another, assisted me. Eight days after, his host, Mr. W——t, and myself, and fourteen days after, the other man, were attacked. This man died. He lived about one mile from the scene of action. Nine days after his decease one of his children* died likewise. On the day of H. S.'s death—the first victim—his oldest

Cases marked thus: (*) signifies, not treated by me.

daughter was attacked. The next day a Mr. H——l took her, from charity, into his house, which was situated about one and a half miles from where she had been taken sick. At this time I did not as yet believe in the contagious nature of the disease. Seven days after, Mr. H. was a corpse. A woman, a relative of his, living about one and a half miles from him, in the country, washed the clothing he died in. Eight days after *she* was dead. Five days more, and two children died in the house this wowan had lived in. A neighboring lady, visiting Mr. H., brought the cholera home; she and two more of her family were attacked. Let us return to the first mentioned house of Mr. W——t. His wife was taken ill with it. A brother visiting this family, and residing over one mile from them, brought the disease home. He and five of this family were attacked; the first one of those attacked died. I brought the cholera into my own house. All my colleagues of this place could adduce a number of cases of like import. We are all of the opinion *that cholera can be, and, at all events, this time has been contagious.* When the contagiousness of our epidemic cannot be denied, the question arises: Why has not cholera been contagious in previous epidemics? I only shall speak of what I have seen, not of what I heard. In 1849 I observed this disease in St. Louis and New Orleans. The latter port was the first affected, whence it spread to many landing places of the Mississippi, until arriving at St. Louis. It was in January of that year there appeared the first cases, *imported* by travelers from New Orleans. Cholera was likewise *imported* from Europe—it was not autochthon. Physicians, persons and nurses attending the sick, were attacked, and many even died of its ravages. Does this, too, speak *against* its contagious nature? Some may say that not all persons attending the sick were attacked, and that this fact proved its *non-contagiousness.* But which *acknowledged* contagion can extend its influence over every person?—How many cases of the most malignant form of variola

have I attended, and how many victims of this loathsome foe dissected, without being injured by it myself! This proves that cholera, like any other malignant disease, observes the law of "predisposition." Not only this, but other causes may have prevented cholera from becoming in one place more than endemic, or only appear sporadically, although transplanted by direct communication, whilst in the place it emigrated from, it raged as an epidemic. They may be double. To answer the first, I shall quote here part of an article I wrote in 1850.

As nature never transgresses her laws, cholera must have its germ. *I do not doubt but this ens proprium associates with malaria, but doubt that malaria, without the germ of cholera can become cholera proper.* Therefore yellow fever, and all the host of specific diseases malaria is charged with producing, must have their respective germs, which undoubtedly enter into alliance with malaria. Nay, more: I firmly believe that malaria is the nucleus of these different germs; that it only stands in such a relationship to them as the *albumen* to the *vitellum* of the egg. *Ergo, to produce malaria, sulphates are required. Now, if a vicinity or place is not supplied with these bodies, (sulphates of lime and of magnesia,) malaria cannot arise.* Let us suppose malaria and sulphuretted hydrogen the immediate causes of cholera, how could it be possible for these to produce such a diversity of disease? I acknowledge it possible that different degrees, but only of one and the same disease, could be produced, according to the greater or smaller quantity generated. But there are places which contain either more or less of sulphates, and the disease, as an individual hardly varies in intensity. It is possible that when the germ of cholera is introduced, some places are perfectly exempted, whilst others are nearly devastated. Again, at a time of recurrence of the scourge, the first place may have acquired the conditions necessary, whilst the other has not. The second cause is ·vant of predisposed persons to increase the disease to

an epidemic, when the contagion necessarily becomes extinct. Now we inquire about the nature of contagion.—First, what is contagion? This name we apply to all those noxious products generated in the animal organism, which, when transplanted into an organism of as high a standing as that in which this deleterious matter has been produced, will cause absolutely the same organic changes—*disease*—as that which caused its genesis. We shall now pass to the genesis of *epidemics*, from which we will deduce the genesis of *contagion*.

When the atmospherical and telluric noxious influences produce upon the organism so great a chemical change as to cause a disturbance of functions of the body, then we call this abnormal state disease. The abnormal, like the normal state, has its products and educts. When the functions are changed, the organic structure must change likewise; ergo, the products and educts of the body likewise. The more intense the organic and functional changes are, the greater abnormity the products will exhibit. When there occurs one or more cases of the same lesions, the same external symptoms,—also of the same disease, we call them "spontaneous." Should those telluric or noxious influences extend over a larger range of country, causing disease, we say it is endemic; but if they extend over still larger districts, attacking a very great number of individuals, we say the disease has become an epidemic. When during the prevalence of an endemical or epidemical disease, many persons attacked by it are enclosed in relatively small rooms, to which not enough fresh atmospherical air can be admitted, then these rooms will not be filled with carbonic acid, but *with animal products of exhalation*, and perhaps with those of the voided excretions, which will now putrify, and not only produce the original disease, but by far a more virulent one, causing, in some, dyscrasy of the blood, but in all *affecting the nervous centres*. This new product we call *contagium*, and

the manner in which it originated its *genesis.* It is not necessary that contagium be generated only in enclosed rooms. If many individuals under similar conditions are attacked in a low malarial region, where change of atmosphere, on account of its geographical position, is impossible or difficult—as in valleys to which the wind has no access—and where, perhaps, yet planetary influences, may exist (as the hot rays of the sun,) there contagion will likewise be generated, and will be the more lasting, as it finds a ready nucleus in malaria. This I conceive to be the process in which the contagium of wide spread epidemics is caused.

In this manner change the following and other sporadic, endemic, and epidemic diseases, into *contagious epidemics,* as yellow fever, scarlatina, variola, the plague, tussis convulsivus, *Cholera Asiatica.* A contagion once generated acts entirely different, on account of its more virulent nature, and when carried to other places, appears there as contagious disease, not passing through the same process described, nor needing those peculiar favorable conditions as are required to its production. It appears as contagium, and will exist as such, as long as circumstances mentioned before do not exclude it. The next question to be answered will be: Of what nature is Cholera Asiatica, or to what genus of disease does it belong? My answer is: *It is an intermittens, holding the same relation to the so-named organic nervous system, as intermittens perniciosa holds to the cerebro-spinal system,—in other words, a primary localization of the typhoid process upon the ganglionic nerves, which becomes transmitted secondarily to the central nervous organs.* L. W. Sachs, of Kœnigsberg, holds it likewise for an intermittens,—but I am not acquainted with his theory. My reasons for the identity of both diseases rest entirely upon physiological and pathological resemblances, as also upon the similarity of localities in which both originate, and the frequent change from one into the other. Let us draw a comparison between them:

PLACE OF NATIVITY.

Intermittens.	*Cholera.*
Low marshy grounds on the banks of rivers and swampy places, where malaria can originate.	Low marshy grounds, in the deltoid countries, through which the Ganges passes, and those of Bengal, Ceylon—all malarious districts.

CAUSE OF GENESIS:

MALARIA.

MODE OF ACTION UPON THE SYSTEM:

INTOXICATION OF THE BLOOD.

CIRCULATION.

The blood is removed from the peripheric to the central parts. The contractions of the heart are numerically increased.	The blood is removed from the peripheric to the central parts. The contractions of the heart are augmented.
During the cold stage, the temperature of the lower extremities is sunken, the skin of the fingers corrugated, lips, and sometimes the extremities of a bluish color.	The temperature of the upper and lower extremities is sunken, except that of the thorax and abdomen.—(Symptoms given before.)

NERVOUS SYSTEM.

The seat of this disease is that part of the peripheric nervous system, which enters into the closest connexion with the vascular.	The seat of this disease are the ganglionic nerves, following primarily their nearest nervous plexuses, and attacking, secondarily, through their spinal fibres, the spinal and cerebral organs.
In the ganglia-intermittens we observe a strongly marked cold stage, whilst in the cerebral intermittens it exists only slightly, or not at all. Neither appear in the latter perfect crisis per perspiration and urine.	Here we have the highest expression of the cold stage. When terminating favorably, crisis by skin and urine takes place.
As well as cerebral—may change into ganglia—intermittens, the latter may change into the former. A cerebral intermittens may originally attack the trigeminus, from hence passes to the vagus—where it causes disturbance of respiration—following its ramifications until it reaches the ganglionic nerves, and a ganglionic intermittens, may, in a similar but reversed manner, change into a cerebral intermittens.	Cholera commences in the ganglionic nerves, attacks the abdominal plexus, thence follows the relations of the vagus, causing disturbance of respiration, dyspnœa, and finally settles upon the cerebral organs.
Intermittens may locate upon the central parts of the abdominal nervous system, when it will cause violent emesis and diarrhea, which is physiologically explained by the intimate relation between the ganglionic system, and the abdominal mucous membranes.	Here we have violent emesis and diarrhea, on account of the localization of the typhoid process upon the ganglionic system, and the relations of the latter to the abdominal mucous membranes.
Intermittens is confined to the human species.	Cholera is confined to the human species.
This disease, at a time when cholera is prevailing, may change into cholera. It likewise may change into enterotyphus.	Cholera will change into intermittens, and in some instances has changed into enterotyphus.

NERVOUS SYSTEM—*Continued.*

Intermittens.	*Cholera.*
During the cold stage it may end by paralysis, causing hydrops, one kind of which takes place suddenly, by exhaustion of the abdominal nerves.	Cholera in the first stage ends by paralysis. Here accumulation of water takes place, which is evacuated per mouth and anus.
In some instances we observe a papular eruption—as auxiliary crisis—in the mouth, nose, the mucous membranes, and even over the entire skin, which by the older physicians has been named "critical itch."	In some instances we meet with an eruption resembling roseola, wherefore it has been named "roseola cholaica," which in every case is a good symptom. I met with it in two cases; both were women.

I shall add a few physiological facts, as a proof that cholera attacks, primarily, the ganglionic nerves.

Vagus.—This nerve—not properly belonging to the ganglionic, on account of its intimate connection with this system, I speak about it here—sends many rami to the lungs and the heart. When irritated, respiration becomes difficult and hurried, and the circulation more rapid. When the contractions of the heart of just killed mammalia have ceased, they may frequently be caused again by pressing the cervical portion of the vagus. E. Weber and Budge have most clearly proved the influence of the vagus upon the heart, by their experiments with the magnetic-electrical rotatory apparatus. When G. Valentin mechanically irritated the cervical portion of the vagus in recently killed dogs, cats, rabits, or horses, he observed distinct motions of the stomach. The latter sometimes became contracted, sometimes forming an incisura from the greater to the lesser curvature, and at other times performing wave-like motions from the cardia towards the pylorus. Anatomy shows that branches of the vagus connect with the solar and hepatic plexuses, which fact elucidates the influence of the cervical portion of this nerve upon the large and small intestines.

Sympathicus.—Valentin observed that the cervical portions of the vagus and sympathicus, the inferior cervical ganglion, the rami cardiaci, and cardiac plexus may either produce or alter the contractility of the different portions of the heart. The stomach may be influenced by the

sympathicus into four kinds of contractions, one of which is a peristaltic motion of principally the pyloric portion. When Volkman applied the magneto-electro-motor to the thoracical part of the sympathicus proper, violent peristaltic motions of the stomach and intestines ensued. (Experiments made on an asphyxiated cat.) When the same applied the wires to the ramus splanchnicus major, before it enters the cœliac plexus, strong contractions of the stomach were produced. John Muller, Kuerschner, and other physiologists, have observed that a mechanical or chemical irritation of the plexus cœliacus will produce violent peristaltic motions of the smaller intestines. The lumbar and sacral parts of the sympathicus produce strong actions of the larger intestines. M. Sehiff, in his "De vi motoria baseos encephali inquisitiones experimentales," says that when he had extirpated several of the posterior abdominal ganglia of the sympathicus of a frog, he found the peritoneum and the intestines infiltrated, and the latter and the abdominal cavity filled with effused fluids. The present state of our knowledge of the physiology of the nerves is such as to leave plenty of room for inquiry. When, therefore, many points, as regards the nervous pathological influence in cholera, remain shadowed, we have a right to judge by analogy, from the results of experiments instituted upon nerves in their physiological state, on the pathological condition of these identical nerves by the resemblance of their functional disturbances. Footing npon this right, I judge that *Cholera is a primary disease of the ganglia.* When one pathological process—disease—whose nature is disputed, in its symptoms resembles another, of fixed form and undisputed nature, more than any other, we are justified to classify it with the latter process. Upon this ground I classify cholera with *intermittens.* I have forgotten to state, in the proper place, that I observed *one case of cholera change into intermittens perniciosa, and two cases of the latter become cholera.*

Since I wrote this I have read the report of the Committee on Practical Medicine and Epidemics, (of the American Medical Association for 1852,) on which I shall take the liberty to make a few remarks.

As regards the different curative means which proved, in the hands of many, remarkably successful, whilst others complain of the reverse, it is here the place to say that cholera, like every other disease, differs widely in its nature. It may be simple erethic, inflammatory, or torpid. The *erethic form* calls for the least interference—nature needs only to be guided carefully—whilst the *inflammatory form* requires an energetic treatment. Here antiphlogistics, with which men of high authority have had brilliant success, are in their proper place. The third, the *torpid form*, calls for an entirely different course. Here the antiphlogistic regimen would be downright murder. The cerebro-spinants and roborants are in their proper place. The sinking powers must be reanimated, the apathic nerves called into a new action. The difference in the nature of this disease depends not only upon the cholera-virus as regards quantity and quality, but also upon the subject attacked, upon epidemical and terrestrial conditions. The disregard, or the malperception of these important causes, has been the reason for many failures of remedies, which others, at certain times, found unsurpassable. Here I must remark that those remedies which I found beneficial, may be to others, at later times, perfectly useless. Only the nicest discrimination of local and individual constitution, and of that of the then reigning genius epidemicus morborum, will prevent this. The latter must always be considered, as it is of immense import and regulates the selection of our therapeutical agents. All *our* local diseases partake this year, more or less, of the so named "nervous" type.

Saline treatment.—Although some hospital reporters may praise it, I do not think the method upon which it is

founded a perfectly sound one. The late Prof. Canstatt, of Erlangen, states that experiments made by Dr. Vanderlinden, in the cholera hospital of Brussels, after the method of Stevens, proved not only unsuccessful, but that they seemed to provoke collapsus.

Emetics.—In gastric complications, particularly in the erethic form, they are of a salutary effect, but I think not advisable in *every case.* Their chief eclat, after emesis, consists in their general effects. Hufeland says: "These are due to sympathies, depending upon the gastric nerves. The connexion of these nerves with all other parts of the body, gives great importance to these general effects, and is the medium through which vomits affect every system of the economy."

As regards the contagiousness or non-contagiousness of cholera in St. Louis in 1849, I must say that I saw at least *one* physician of the St. Louis Hospital, intimately known to Dr. McP., attacked with premonitory symptoms. This was in January.

My intentions in writing this essay were to give to the profession facts as they appeared here. When I permitted myself to reason, I must be excused, as the import of the principles involved is strong enough to lead astray.—In some instances I have been guilty of repetition, but only where it became necessary to prove or elucidate by it other facts.

NEW ALBANY, IA., Sept. 15, 1852.

ART. II.—*A Remarkable Case of Reflex Nervous Action.* By J. W. BRIGHT, M. D., Louisville, Ky.

That nervous irritation in one part of the human system, by whatever cause it may have been excited, may be transferred to another part, or organ, is an occurrence so common that every practising physician is fami-

liar with it. For instance, the irritation of a calculus in the bladder will give rise to a pain in the glans penis, or pains in the thighs. Now those sensations are not produced by a direct communication of sensation through the nervous cords, but the physical changes excited by the first, are reflected in the second. Again, when the stomach is affected by the action of a strong acid upon its coats, there is often a severe pain felt in the supra-orbital nerve. An antacid taken into the stomach quickly relieves this pain. A lump of ice swallowed suddenly will often produce a similar effect in the supra orbital nerves, and a draft of water warmer than the ice will as soon relieve it. My object, however, in this short notice, is to give an account of a remarkable case of reflex nervous action which I attended a short time since.

I was called, September 8th, 1852, to see Mrs. B——, about six months and a half advanced in her first pregnancy, a very delicate lady. She had been attacked the day before with a chill, followed by fever of the usual intermitting form, which was, by the use of the ordinary remedies, removed in two or three days. About twenty-four hours after all the symptoms of fever had been removed, pains set up in the uterus, simulating those of labor. I saw her in five or six hours after these pains commenced, her friends supposing that she was in labor. Her pains came on regularly, and were of considerable strength. On examination of the os uteri I found the pain was of spasmodic character, and not genuine labor pains—with nothing of the expulsive character attending such pains. An anodyne was ordered, (morphine and tinct. valerian,) which soon suspended them in the uterus. But in a very short time the spasmodic action was transferred to the diaphragm, and hiccup set in, which became very troublesome, and resisted all the usual remedies for twenty-four hours, till finally it yielded to a preparation of creosote, sulph. æther and laudanum.

But notwithstanding she was under the influence of the above medicine, as soon as the hiccup began to subside the pain returned in the uterus, and by the time the hiccup was entirely relieved, the uterine pains were as severe as at first. I allowed them to continue for eight or ten hours, but no evidences of delivery appeared. The os uteri remained firm, and the head of the child made no descent. I then suspended the uterine pains once more by the use of McMunn's elixir of opium. Sixty drops had the effect, and she immediately commenced hiccuping again; but after a few spasmodic actions of the diaphragm the action passed to the brain, or membranes of the brain, and she fell into a profound stupor. In this state her pulse was one hundred and five in a minute, soft and compressible; her breathing only nine times in a minute. She could be aroused by shaking and talking loud to her, but would instantly fall into a deep stupor again. The pupil of the eye at this time was very much contracted, and not sensible to light. There was frequent knitting of the eyebrows. She could talk and swallow when aroused, but the pulse was too soft to bear the lancet. I ordered an active cathartic, to be followed by an enema. This operated well, but no relief was obtained. I then ordered strong cataplasms of mustard to be applied over the stomach and extremities, for they were too cool, and gave a full dose of calomel, and again met it by an enema, which also acted well. No relief to the brain. I then resolved to excite the uterus, and as soon as I effected it, and the pain was set up there, and labor commenced, she began to arouse and talk, and look about the room. In six hours from the time the uterus was again in active operation the child was born, and she was fully awake, and perfectly conscious of every thing that was passing.

Thus, it will be remarked, forty-eight hours passed before the uterus was made to act, and her brain relieved.

The child lived about twenty-four hours. The mother recovered without any difficulty. The remarkable character of this case has induced me to give it to the profession. The treatment is nothing more than any experienced member of the profession would have adopted under such circumstances. It is the reflex action of the nervous system shown in this case to which I wish to direct the attention of the profession. I leave it without comment. Although I have practised medicine for nearly forty years, I never saw exactly such a case before. I am induced to believe that they are rare, and, when they do occur, we need not expect relief until delivery is accomplished.

LOUISVILLE, October, 1852.

ART. III.—*A Case of Encysted Tumor Extirpated.* By. M. J. BRAY, of Evansville, Ia.

Mr. James Smith, residing in the country, came to Evansville to have a large tumor removed from the neck. He had, naturally, a good constitution, but at this time, and for two or three months previously, he had suffered from the irritable effects of the tumor. He was somewhat emaciated and reduced in strength, and had a slight fever every night, which disturbed his rest.

He was about forty years of age. The tumor made its appearance sixteen years ago, and has since gradually increased in size. Its base was composed of conglomerations of adipose tissue, but the middle and apex appeared to be encysted.

It had become complicated in its organization in consequence of repeated attacks of inflammation. It was situated on the head and neck, occupying the whole of one side, extending from the spinous processes of the cervical vertebra to the median line of the neck in front,

It measured twenty-six inches in circumference, and thirty-one inches from its base on one side, over its apex, to the other, and weighed, after it was removed, twelve pounds.

The operation lasted three quarters of an hour. The arteries were ligated as soon as they were divided.—Seven or eight required the ligature, and the strength of the patient was supported by the liberal administration of brandy, given in small doses.

After the tumor was removed, the flaps of the wound were placed in coaptation, and secured in the usual way. A large proportion of it united by the first intention, and in ten days from the time of the operation, the patient was able to return home.

I think this tumor is as large as any, if not the largest, which has been successfully extirpated in the United States.

EVANSVILLE, IA., October, 1852.

REVIEWS.

ART. IV.—*Clinical Reports on continued fever, based on an analysis of one hundred and sixty-four cases; with remarks on the management of continued fever; the identity of typhus and typhoid fever. relapsing fever, diagnosis, etc. To which is added a memoir on the transportation and diffusion, by contagion, of typhoid fever, as exemplified in the occurrence of the disease at North Boston, Erie county, N. Y.* By AUSTIN FLINT. M. D., Professor of the Principles and Practice of Medicine, and of Clinical Medicine, in the University of Buffalo, and Editor of the Buffalo Medical Journal. Buffalo: George H. Derby & Co., 1852.

Dr. Rush was wont to say that a man might as well attempt to portray a summer cloud in a painting, as to attempt to describe a fever. The elements of the disease are undergoing perpetual changes, and Dr. Rush looked upon any attempt at seizing and fixing the fleeting movements as an attempt at impossibilities. Those who have devoted their lives to the perusal of treatises on fever, may easily conclude that Claude, of Lorraine, was more successful in fixing the features of summer clouds on canvass, than writers on fever have been in portraying its changing forms. Doctors differ widely in their views, and their differences have given point to a sarcasm. But these differences exist in the varied phases of the human mind, and are not confined to the medical profession. Two artists may view the same landscape, and in their attempts to transfer their impressions to canvass, may

differ so widely that it would be difficult for any one to detect a resemblance. Yet, each may have been honest in his attempts, and faithful in executing his conceptions. Fixed as the features of a landscape may seem to be, a thousand circumstances may change the appearances that strike the eye. That which was dressed in peculiar loveliness, while the first beams of the morning sun bathed themselves in the glistening dew, or lighted up the glossy green of the grass, or enlivened the beauty of the flowers, would, in the roseate blush of twilight, look like a different scene. While the bloom of the Drummond Phlox is in repose, it presents a uniform mass of red; but if the passing zephyr shall curl its beauties, the prismatic colors might envy the varied hues that dance upon the retina of the eye. It is evident, then, that men may differ without mistaking matters. If we wish to do their differences justice, and drink in truth ourselves, we must know how each conception was formed; we must know from what point each viewed the landscape, and the circumstances that governed the view.

If in such a comparatively permanent fixture as a landscape, these considerations should weigh, how greatly must they be extended when they come into the range of fever, where all is change; where multitudes of circumstances are fixed in nothing but their power to cause variableness. The time may come when all this variety may classify itself, and men may be able to arrange the movements of changeableness into positive groups, as the geologist does his formations. Confusion once reigned supreme in geological science, where now the human intellect has planted beacons that give perpetual light; and if the proper methods of observing and analyzing fever are kept up, we doubt not the path of progress will be well worked.

In the treatise before us, Professor Austin Flint has presented a notable example of patient observation and

industry, of clear and comprehensive views, and of rigid analysis. In undertaking to exhibit his labors, we wish it to be distinctly understood that it must not be taken for granted, that merely because we do not at any point controvert the views or inferences of the author, we necessarily agree with him. Our main object is to point the attention of the reader to the views of Professor Flint, and to show what they are, and occasionally to refer to the foundations on which they rest.

The Clinical Reports originated in the fact that at the annual meeting of the New York State Medical Society, in 1850, the scientific subject which elicited most discussion was the identity or non identity of the forms of fever now commonly known as *Typhus* and *Typhoid;* and a committee was appointed on which the author was placed as chairman, to collect facts relating to Continued Fever, and report at the next annual meeting. The assignment of this duty led the author to examine the cases occuring under his own observation, the histories of which he had recorded; and finding the number considerable, he concluded to subject them to numerical analysis,—and these, for good and sufficient reasons, were published in successive numbers of the Buffalo Medical Journal, and afterwards presented in that shape as a report to the society. In the autumn of 1850, and winter of 1850–'51, another series of cases passed under observation at the *Buffalo Hospital of the Sisters of Charity*, where most of the cases forming the basis of the report just mentioned had been observed. In order to compare the results of the analysis of this second collection with those of the first, the author undertook the labor in the summer of 1851, and published the results in the Medical Journal in which the first series appeared. This second collection consisted of forty-eight cases. As a supplement to these reports, some remarks were contributed on the "Symptoms of Typhoid and Typhus Fever; the identity

of these two types; the diagnosis, etc." A paper on the "management of continued fever," was also added. In the autumn of 1851, and the winter of 1851–'52, another series of cases more numerous than before, passed under the observation of the author. This series constituted the "Third Clinical Report on Continued Fever, based on an analysis of sixty four cases." Appendices were added to the third report. The first is on the morbid condition after death, (exclusive of intestinal lesions,) distinctive of Typhus and Typhoid fever. The facts in this paper are derived from Dr. Jenner's publications in the Edinburg Monthly Journal of Medical Science, in a series of articles commencing with the No. for April 1849, and ending with the No. for April 1850. This series presented an analysis of sixty-six cases of Continued Fever, embracing the two types, Typhoid and Typhus, "the object being to settle the question of the identity or non-identity of the two types by a comparison of the phenomena presented during life, and disclosed with the scalpel after death. The second appendix relates to *Relapsing* fever, and presents the results of an analysis of fifteen cases.

The closing paper of the work is devoted to a "Memoir on the Transportation and Diffusion by Contagion, of Typhoid fever, as exemplified in its occurrence at North Boston, Erie county, New York."

We have thought it due to the author, and to the important subject to give a minute detail of the sources of information that form the basis of the work. No one can doubt that the course pursued by Professor Flint is the correct one for shedding light upon a subject which has many points of obscurity in it, notwithstanding the vast number of tomes that have been written for the special purpose of clearing up all obscurity. The reader of the present work will find himself in the hands of a remarkably intelligent observer, whose teachings deserve and must command respect. The work is in fact a series of Clin-

ical lectures in which an able observer discourses clearly and intelligently upon the phenomena of disease, as they spring to view in the progress of the disease, and they are filled with a fund of instruction that cannot well be taught even at the bed-side.

We do not purpose to enter into an analysis of the statistical details made by Professor Flint. The mere statistics could not be made very profitable to the reader, without an ample reference to the details on which the statistics are founded. Dr. Flint has so perfectly interwoven the various parts of his subjects, that it would be difficult to select any particular part as a sample. But we hope that those who desire to understand all that can be known about the types of continued fever, will not neglect the important contribution made to the subject in this work. It is only by the study of every possible phase of this disease, under the observation of honest, intelligent, and reliable observers, that any one can hope to recognise the ever-varying forms it assumes. These Clinical Reports are very full, and will well reward the diligent student. In order that the reader may see something of the completeness of the plan of observation, we present the elements of the various sections;

Section 1. Age; occupation; civil condition; nativity; habits; season; constitution and previous health of the patient; period of residence in this country, and in this city; duration of the disease before coming under observation.

Sec. 2. The access; Its duration and symptoms; circumstances supposed to have been concerned in the production of the disease.

Sec. 3. Symptoms referable to the general aspect; expression of countenance; decubitus.

Sec. 4. Symptoms referable to the nervous system; mind; sleep; coma; senses and sensibility; muscular contractions, etc,

SEC. 5. Symptoms referable to the digestive system; appetite; thirst; tongue; sordes; parottitis, nausea and vomiting; alvine djeetions; tympanitis; tenderness of abdomen; gurgling.

SEC. 6. Cutaneous eruptions.

SEC. 7. Symptoms referable to the respiratory apparatus; cough; expectoration; pain in the chest; pneumonitis; aberration of respiratory movements; epistaxis; singultus.

SEC. 8. Symptoms referable to the circulation; pulse; capillary congestion.

SEC. 9. Symptoms (exclusive of eruptions) referable to the skin.

SEC. 10. Symptoms referable to the genito-urinary system.

SEC. 11. Duration of the disease; circumstances attending convalescence; sequelæ; mode of dying; fatality.

SEC. 12. Examinations after death.

SEC. 13. Treatment.

SEC. 14. Cases of doubtful type.

The reader will at once perceive, even from these indicæ, how very comprehensive a scope Dr. Flint has taken in making these Clinical Reports. The objects of investigation, embraced in these subjects, belong to each of the three Reports.

The reader may be desirous of knowing the course of treatment pursued by Professor Flint, and to that we direct attention. The means used are distributed under their appropriate heads.

In *two* cases, both of the *Typhus* type, detailed in the third Clinical Reports, no medicinal remedies were employed. Sanitary measures alone were ordered, and the cases terminated favorably.

Laxatives. These were prescribed in several cases where there had been no dejection for several days. No active cathartic was administered in any case. Half ounce doses of castor oil by the mouth, or in simple

enemas, were the laxative measures. The oil was used in ten cases. Nine of these were of the typhus type. In no case, but one, was the oil given more than once during the course of the disease. In nearly every case it was given early in the disease. A single enema was prescribed in *three* cases, all of the *Typhus* type. In one case where castor oil was given, and in one in which an enema was resorted to, the dejections which followed were so frequent that restraining means were necessary.

Anodynes. These were employed in a few cases to restrain diarrhœa, and the undue operation of laxative remedies. The tinct. opii. was used, taken either by the mouth or by enema. It was resorted to in five cases of *Typhus*, and eight cases of Typhoid. The use was very limited. The author had been accustomed to the prescription of anodynes to relieve mental aberration, and promote sleep. Even where vigilance or delirium was not a prominent symptom, Dr. Flint had supposed an anodyne influence salutary. But in the class of cases embraced in the third clinical report, anodynes were resorted to exclusively for restraining undue activity of the bowels, and the sense of their necessity for any other purpose was lessened by the observation of cases in which they were omitted.

Astringents. Tannic acid was given in only *one* case of *Typhoid*, to restrain diarrhœa.

Sedatives. The articles employed under this head were *tartarized Antimony* and *Potassa*, and *Camphor*. The former was used in *three* cases of *Typhus*, and in one case of *Typhoid*. In the first class it was given in doses of 1–16th of a gr., to relieve delirium. In the *Typhoid* case it was given for pneumonitis. In one case of *Typhus*, the antimony constituted the sole medicinal treatment. *Camphor* was given in three cases of *Typhus* for taxic symptoms, and in no case of *Typhoid*.

Ammonia. The carb. of Ammonia was given when-

ever the circulatory forces were failing. It was used in all the cases ending fatally by Asthenia. It entered into the treatment of six cases of *Typhus*, and four cases of *Typhoid.*

Quinia. This was prescribed in doses of *three* grains, three times daily, and continued several days in *four* cases of Typhus. In each of these cases it was given from the commencement of the career. It was given in one of Typhoid.

Huxam's Tinctnre was prescribed in *one* case.

Vesication. Blisters to the nape of the neck were resorted to whenever spasmodic inspiration was observed, or a tendency to coma. They entered into the treatment of all cases that ended fatally by apnœa. The only exception of their use for other symptoms than those named, was their application behind the ears for external otitis. They were applied to the neck in *nine* cases of *Typhus*, and in four cases of *Typhoid.*

Sinapisms and stimulating liniments. Occasionally employed where pneumonitis was the complication, or where coma existed or was threatened.

Diffusible Stimulus. Brandy was almost the sole agent employed as a diffusible stimulus. It entered into the management of most of the cases of both types. It was omitted in but five cases of *Typhus*, and two of *Typhoid.* In several of the Typhus cases (in all nine) brandy constituted the sole remedial agent employed. The quantity given was generally half an ounce; in some cases an ounce. The intervals of its administration varied from half an hour to four hours. In a large proportion of cases, the remedy was commenced early in the disease, and continued to its close. Prostration, coolness of the surface, feebleness, and, more especially, notable frequency of the pulse were regarded as indicating its free administration. But it was often given to forestall, by sustaining the vital forces, the occurrence of these symptoms.

Nutriment was systematically administered, such as essence of beef and milk porridge. The articles of nourishment were given sometimes against the inclinations of the patients. Cleanliness, ablutions and ventilation, were resorted to.

The only attempts at cutting the disease short, were made with the *wet sheet*, and it was used in two cases of *Typhoid* fever without any useful result.

These various elements of treatment exemplify, therapeutically, he treatment of *Typhus* and *Typhoid* fever mainly by stimulants and nourishment. The rate of mortality was not large.

We present the following synopsis, inDr. Jenner's language, in order that our readers may see what that eminent practitioner has to say on the important subject of morbid appearances distinctive of Typhus and Typhoid fever, based on an analysis of sixty-six cases:

"*Cadaveric rigidity*, ceased much more quickly in subjects dead from typhus than from typhoid fever."

"*Discoloration of the walls of the abdomen and of the skin covering the larger veins*, was much more frequently present in those dead from typhus than typhoid fever."

"*Emaciation* had made greater progress in the typhoid than in the typhus subjects."

"*Spots.* The spots observed during the progress of the cases of typhus fever continued after death; no trace of the spots visible during life could be detected after death from typhoid fever."

"*Head.* After typhoid fever, the pia mater and arachnoid separated from the convolutions with abnormal facility in one only of nine cases examined with reference to this point. The vessels of the pia mater were abnormally filled with blood in one-third of the cases, but intensely congested in one only of the fifteen cases; the cerebral substance was eongested in one-seventh of the cases. After typhus fever, the pia mater and arachnoid separated with abnormal facility in nine of eleven cases of which notes on the point were made. The vessels of the pia mater were congested in nearly half, and intensely con-

gested in one-fifth of the whole of the cases; while the cerebral substance itself was abnormally congested in half."

"*Hemorrhage into the cavity of the arachnoid*, which was not found in a single case of typhoid fever, had occurred before death in one-eighth of the cases of typhus fever." "The amount of serosity found within the cranial cavity was decidedly greater after typhus than typhoid fever."

"*Pharynx.* After typhoid fever, this organ was found ulcerated in one-third of the cases. After typhus fever, ulceration of the pharynx was not detected in a single case."

"*Larynx.* Ulceration of the larynx was found in one of fifteen subjects dead from typhoid fever; in one of twenty-six from typhus fever."

"*Œsophagus.* After typhoid fever, ulcerated in one of fifteen cases in which it was examined. After typhus fever, the œsophagus was free from ulceration in all the twenty-four cases in which it was examined."

"The epithelium separated from the œsophagus spontaneously at an earlier period after death from the latter than the former disease."

"*Stomach.* In none of the fifteen cases examined after death from typhoid fever was the mucous membrane of the stomach softened throughout the whole extent; in no case did softening of the cardial extremity approach perforation. In four of thirty-seven cases of typhus fever the whole mucous membrane of the stomach was softened; and in four others there was such extreme softening of the whole of the coats of the great *cul de sac*, that they were perforated by the slightest violence.

"*Small Intestines and Mesenteric Glands.* The presence or absence of lesion of these organs was the ground on which the cases of typhoid and typhus fever here analyzed were divided from each other—consequently they were invariably diseased in the one, and sound in the other."

"*Large Intestines.* After death from typhoid fever the mucous membrane of the large intestines was found ulcerated in rather more than a third of twenty cases In no instance after death from typhus fever.

"*Peritoneum.* As peritonitis was in typhoid fever secondary to, and dependent on the entero mesenteric disease, it may here be excluded from consideration."

"*Spleen.* This organ was enlarged in all the cases of typhoid fever—softened in one-third of the cases only. Before the age of fifty, it was as large after typhus as typhoid fever; after that age, it was decidedly smaller in the former than in the latter affection. After the age of fifty, it was as soft in typhus as in typhoid fever; before that age it was frequently softened."

"*Gall Bladder.* There was ulceration of the lining membrane of the gall bladder in one of fourteen cases of typhoid fever; in none of thirty-one cases of typhus fever. In the latter disease the bile was much thicker, and of a darker green color than in the former."

"*Liver, Pacreas, Kidneys.* These organs were more flabby in the cases of typhus than in those of typhoid fever."

"*Ordinary Bladder.* This viscus was ulcerated in one of the cases of typhoid fever—in none of the cases of typhus fever."

"*Pericardium.* This cavity contained a small amount of yellowish transparent serosity in all the cases of typhoid fever examined. The contained serosity was red, from transudation of a solution of hæmatosin, in five of thirty-one cases of typhus fever, in which the pericardium was examined before the termination of the fever."*

"*Heart.* The muscular tissue of this organ was much more frequently and decidedly flabby, and its liniag membrane was much more frequently and deeply stained of a dark red color, in the cases of typhus fever, than in those of typhoid fever."

"*Lungs.* Granular and non-granular lobular consolidation were very frequent in the subjects dead from typhoid fever—rare in those dead from typhus fever. The reverse was the fact with reference to congestion of the most depending part of the lung."

"*Pleura.* Recent lymph or turbid serosity was found in six of fifteen cases of typhoid fever—i. e., between half and one-third, or in the proportion of forty per cent.

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* There is evidently a mistake in the last sentence.

The same lesions not much less in amount were found in two only of thirty-six cases of typhus fever—i. e., one-sixteenth, or in the proportion of fifty-five per cent."

The foregoing is copied from the Monthly Journal of Medical Science for April, 1850.

Dr. J. adds, after the foregoing parallel of pathological appearances, as follows: "The particulars here briefly recapitulated, and still more those fully detailed in the foregoing papers, appear to me to prove indisputably that the symptoms, course, duration, anatomico-pathological lesions, and the tendency to cadaveric changes, are different in typhoid fever from what they are in typhus fever."

On the subject of relapsing fever we should like to present a full analysis of the views of Professor Flint, but we must content ourselves with snch a notice of this form of fever as will enable our readers to observe and detect it. We have seen several cases of this apparent mongrel variety of fever, and its phenomena are at times very embarrassing. The following is the synopsis made by Professor Flint:

Evidences of a form of fever supposed to be identical with *Relapsing fever*, have heretofore received different names, such as the "short fever;" the "five day fever;" the "seven day fever;" "bilious remittent fever;" "remittent icteric fever;" "mild yellow fever," etc.

The disease is stated to affect all ages, and both sexes in about an equal ratio.

The *access* does not present any very distinctive features. The attack is oftener abrupt than in typhoid fever, and the muscular and articular pains are apt to be severe.

Delirium, and other head symptoms are represented to be oftener absent, and, when present, less in degree than in the other forms of continued fever.

The absence of the abdominal symptoms which are so generally present, in a greater or less degree, in *typhoid* fever, is an important point distinguishing *relapsing fever*. Diarrhœa, with notable meteorism, and tenderness in the

iliac regions, do not belong to the latter. It is, however, characterized by certain symptoms pertaining to the digestive system rarely found in *typhoid*, viz: nausea and vomiting, which are frequently prominent symptoms, and tenderness over the epigastrium. The matter vomited is stated to be "bright grass green," and sometimes like coffee grounds, approximating to the black vomit of yellow fever.

The eruptions characteristic of *typhus* and *typhoid*, are not found in *relapsing* fever. Sudamina and occasionally petechiæ have been observed. An eruption consisting of "small spots, round, purple, unaltered by pressure," has been described by some observers as an element of the affection. These spots are stated to be extremely like flea bites, and it does not appear to be fully ascertained that they were not, in fact, flea bites. The chest symptoms, i. e.: cough and bronchial rales, so generally present in *typhoid* fever, are said to be less commonly present in this disease. *Epistaxis*, so frequently observed in *typhoid* fever, sometimes occurs. The pulse, seldom falling below 100, in more than half the cases is 120, and in a considerable number the frequency is still greater.

Profuse sweating is stated to occur pretty uniformly, preceding the apparent convalecence and also when the relapse of fever is about to terminate. Another distinguishing feature referable to the skin, is more or less yellowness, occuring on the fourth or fifth day. In severe cases the jaundice is often a prominent symptom. The most distinctive event, however, is that indicated by the title of the disease, to wit: the occurrence of relapses. The first febrile career continues for a period rarely less than *four*, nor more than *ten* days, and then ends, generally after profuse sweating, which is considered to be critical, leaving the patient free from febrile movement, and apparently convalescent. After an interval varying, in duration from *five* to *eight* days, another attack occurs, generally abruptly, and frequently preceded by a chill.—The recurring febrile movement is equally or more severe than the first. It continues from four to five days, and then terminates usually after profuse sweating. Usually after a single relapse, the patient becomes permanently convalescent. A second, third, and even a greater number of relapses, however, have been observed to occur.

The disease is seldom fatal. Patients almost uniformly recover, unless some complication takes place, such as pleurisy, pneumonitis or dysentery.

The most distinctive characters derived from autopsical examinations, are the absence of the intestinal lesions belonging to *typhoid* fever. The peyerian glands are unaffected. The spleen is generally enlarged and softened.

It may be communicated by contagion, and Dr. Jenner, in the article already referred to, has collected observations tending to show that relapsing fever cannot be derived from patients laboring under the other forms of continued fever, but that it alone produces the special miasm for its propagation. How far the facts contributed by him go to establish this important point, it does not fall within my present purpose to inquire.

This form of fever does not secure immunity from the other forms; nor from subsequent attacks of the same form.

These are, briefly, the more prominent of the traits distinctive of *relapsing* fever which are mentioned by the two writers to whom I am indebted for the foregoing summary. They appear to have been deduced from an examination of reports of epidemics, supposed to be of this character, by various authors. If they are sufficient to render probable the position that the disease to which they relate is a peculiar form of fever, distinct from the *typhus* and *typhoid* forms, it is certainly desirable that collections of cases should be subjected to careful numerical analysis. The question can only be definitely settled by the results of such analysis.

So far as I know, we have no account of a form of fever prevailing in this country, analogous to the relapsing fever as just described. Were the cases characterized by relapse, among those observed by me during the winter of 1850–51, cases of *relapsing* fever? With a view to arrive at the answer to this question, and, if the conclusion be in the affirmative, to furnish a small contribution toward the natural history of that form of fever, I will now proceed to analyze the cases referred to.

We must here close our analysis of this valuable contribution to the subject of Fever, and pass to the consid-

eration of Professor Flint's paper on "The Transportation and Diffusion by Contagion of Typhoid Fever." There is nothing about Typhoid fever that has called forth a greater variety of opinions than the means of its transportation and diffusion. How far the epidemic character of Typhoid fever may sustain the doctrine of contagion, we are not prepared to say. We must frankly say that we have never seen any thing, under our own observation, that would justify a belief in contagion. We think the disease is somewhat rare in Louisville, because we have seen but little of it here in the course of many years. Very recently we saw four cases in one family. In this instance we know of nothing that would lead any one to suspect contagion. There were no cases from whence these subjects might have caught the disease, and numerous children visited their sick friends, but no one of them received any infection. There were young members of the family exposed to whatever there may be of infection in the disease, but they did not suffer from the exposure. These are facts that we observed, but we do not urge them as militating against the doctrine of contagion; we mention them only as facts observed. They are too limited either to establish or destroy a doctrine.

It is evident, however, that the belief in the contagiousness of Typhoid fever is growing, and we are assured that there are observers, who commenced making memoranda, as anti-contagionists, who are surprised to find facts accumulating among their records, that strongly point to contagion. While we remain unconvinced, we acknowledge that there are appearances in favor of the contagiousness of the disease, that cannot well be accounted for or explained if we deny the doctrine of contagion. The paper of Professor Flint, of which we are about to make a synopsis, has already converted former skeptics, and fixed the faith of some who were wavering.

It is due to the subject that the facts drawn from the fever of North Boston, Erie county, N. Y., be fairly presented.

North Boston is about eighteen miles from Buffalo, and twelve miles from the shore of Lake Erie. It was a salubrious spot. No malarious source was known in its neighborhood. Neither Intermittent fever, nor any of its kindred types, had prevailed for several years. Typhoid fever had never appeared in any part of Erie county. Remittent fevers had been the usual types, but these have diminished, and Typhoid fever has proportionally increased.

In 1843, the year of the fever, there were but nine families in North Boston, all living within an area of an hundred rods in diameter; but the few houses in which the disease occurred were closely grouped together, the house farthest removed from the tavern being only ten rods distant. Forty-three persons made up the entire community.

In September, 1843, a young man from Massachusetts being on a journey westward, took lodgings at Fuller's tavern. He had been ill for several days, and stopped at North Boston because he was unable to proceed any farther. He remained at the tavern, and died on the 19th of October. The symptoms establish the fact that it was a case of *typhoid* fever. In the portion of Massachusetts from which the stranger came that is the prevailing form of fever.

Between October 19th and December 7th, twenty-eight of the forty-three inhabitants of North Boston were attacked with fever, and ten of the cases proved fatal. The first case was in a son, aged sixteen, of the inn-keeper. He was attacked twenty-three days after the arrival of the stranger. About the same time a daughter of the inn-keeper was taken ill with the fever; then another daughter, about twelve days later, and another aged three years, about the same time. Another son of the inn-keeper was attacked on the 9th October. Two othe

cases occurred in this family. Seven cases, exclusive of the stranger, occurred in this family.

A son of the inn-keeper, a married son, living about four rods from the tavern, who visited his father's in order to assist in nursing, was attacked on the 15th of October.

About eight rods from the tavern a family of the name of Penhallow was attacked. In the family of Ingraham, residing about three rods from the tavern, were seven cases and three deaths. A family named Hallick resided about ten rods from Fuller's, and seven cases occurred in this family, of which five were fatal. A family named Dobbs, about three rods from the tavern, had four cases, all of whom recovered. Another case occurred in a family living within twenty feet of the tavern.

Only three families in the settlement escaped. One of these lived in a part of the house occupied by the inn-keeper's married son. This was the only family in the restricted area of the disease that escaped. Another family, named Sprague, lived about forty rods from the tavern. Bailey's family also lived about forty rods from the tavern.

In the families that were attacked there were individuals who escaped. Their names and ages are as follows: Fuller, the inn-keeper, about 50; Ingraham, about 50; Dobbs, about 45; Penhallow, about 40, and Hallick, about 40. Fuller was in comfortable circumstances. The other families were poor, almost to destitution. The families who escaped were in good circumstances.

In the mysterious circumstances attendant upon this crushing and new calamity, the imagination rioted.—Wiseacres, as usual, settled, to their own satisfaction, a problem that puzzled philosophers. Untrammelled by those absurd rules that logicians call laws, these popinjays jumped to conclusions without undergoing the tediousness of a journey of ratiocination. Their dictum was sufficient, and all were fools who did not leap over

such things as facts, and seize their doctrine without the trouble of reasoning. These jumping sages found the cause of this disease of New Boston in a poisoned well, just as their similitudes in the dark pestilences of Europe always found the cause of epidemics in water-fountains. They probably had heard that truth lies in the bottom of a well, and for either that reason, or probably for not as good a reason, located the origin of pestilences in wells and springs.

In New Boston the family of Fuller, where the disease commenced, and the family of Stearns, who escaped an attack, were engaged in a bitter feud, and as the disease was ascribed to poison, administered by Stearns, all the families of the settlement cut the acquaintance of his family. A well, near the tavern, was used by all the families except that of Stearns and one other family. It was charged that Stearns had poisoned the well, and futile efforts were made to empty it with pumps. Up to a short period before the outbreak of the disease, the family of Steanes had been supplied with water from Fuller's well, but, owing to the feud referred to, the family of the former had been cut off from the well, and had been forced to make a well for themselves. Another of the three families that escaped did not use the water of Fuller's well. By the others it was used daily. Upon these coincidences the assumption was formed that Stearns had poisoned the well. Some of the water was taken to Buffalo and analysed. The water was found to be remarkably pure. The excitement against Stearns was of course very great, and he instituted a prosecution for slander, which was settled by the party prosecuted by the payment of one hundred dollars.

The mysterious character of the disease, its unusual phenomena, and the poverty of the sufferers, called for medical aid from Buffalo, and Dr. Flint visited North Boston. He satisfied himself by extended observations,

and patient examination, that the disease was Typhoid fever. Autopsies revealed the characteristic lesions of Typhoid fever.

Dr. Flint supposes that the miasm of contagion is much more abundant, or potent, at certain times and places, than when the disease ordinarily prevails. We know that this is the case with the miasm of small pox, for that seems to be almost governed in its virulence by determinate periods. "Or," says Dr. Flint, "it may be contended that the miasm, which generally is inoperative, becomes more efficient; not by any increase of its strength or quantity, but by the co-operation of other agencies, which favor its operation, or render individuals more susceptible to its influence."

We admit that the facts bear strongly towards the doctrine of contagion, but there are facts, even in this strong array, that seem to tell against it. We know of no contagion that pays respect to age; and a number of persons, of forty years and upwards, escaped an attack, though they were in the very focus of exposure. It strikes us that this is an obstacle to the fullness of the proof of contagion at North Boston. The facts and arguments of Professor Flint are stated with clearness, force and entire honesty.

We cannot quit this book without giving our cordial thanks to Professor Flint for his valuable contribution to the study of fever. He has acquitted himself with the highest degree of credit, and we commend his work as one of the best monographs that has appeared on the subject.

ART. VI.—*Report of the Medical Dipartment of the University of Pennsylvania, for the session of* 1851–2 *to the alumni of the school.* By the MEDICAL FACULTY.

There are some curious features in the document, the tiile of which we have given above, that naturally challenge the attention of the medical profession. The American Medical Association has suggested, as a measure of reform in medical teaching, the prolongation of the lecture terms of the schools to six months, and the medical department of the University of Pennsylvania leaped into the measure with a celerity that bordered on bustling. Year after year she made the air sonorous with her bugle notes on the subject, and from her importunities to the American Medical Association, she seemed to have taken up the idea that the sole object of the existence of that body was to hold up the hands of the good old Moses of medical reform, in the battles with the Ammonites. She constantly occupied the annual meetings of the Association with her bills of charges for the great sacrifices she had made in furthering the suggestions of the Association for reform in teaching.

But a change has come over the sturdy champion of long terms. The circular recently issued by the faculty, of what is known as the old school of Philadelphia, thus announces her position in the arena of reform and competition

"It is well known to the friends of this school that it has been engaged in an earnest effort to improve the system of medical education in the United States. One of the means by which it has sought to accomplish this object has been the prolongation of the scholastic term of instruction."

The reader must remember now that in our quotation the object and motive which led to the prolongation of the lecture term are set forth, and that the claim is urged that the object was exclusively the improvement of the

system of medical education in the United States. It is very important that this shall be remembered, for, if forgotten, the reader cannot appreciate the curious attitude that "circumstances," as potent sometimes in medical as in political circles, have forced the University of Pennsylvania to take.

In 1849–50 the term of that school had been extended to six months. By some this prolongation was looked upon as a vigorous stride towards improvement. Others took a reverse view, and we were among the number.—The Harvard school opposed the extension, and based her opposition on ample reasons.

But the school of the University of Pennsylvania claimed that she made the prolongation in obedience to an earnest wish to improve the system of medical instruction in the United States. She annually filled the ears of the profession with testimonials of the gratification she experienced from this great reform. Her evidence to its vast merits was unqualified. But, at the same time, her faculty announced that they were quite aware of the risk incurred; but as no improvement could be expected unless some school would venture to take the first step, "ours, as the oldest, and among the most prominent, seemed especially called on to set the example." Thus it was that a sense of duty pushed the school towards the fires of martyrdom, and as the first effects were found to be "excellent," in the estimation of the faculty, that body urged the general adoption of the prolongation with a vehemence quite worthy of the holy cause. Even in the present document the faculty remark that they are gratified in being able to say, as they do with perfect sincerity, that "there has been a progressive improvement in all respects, increasing steadiness of deportment, attention to study, and earnestness of self-improvement have been observable; and the examinations, though not less strict than formerly, have each year resulted in

relatively fewer failures to attain the honors of the Institution." Here then we have a most satisfactory result of reform; the object of the reform being the improvement of Medical Pupils, seems to have been fully realised by the University of Pennsylvania. The Faculty had felt that their Institution was a city set on a hill, whose example was essential, and whose light should not be hid. They felt that they were adventuring, but were encouraged by duty; and according to the Faculty, the great reform answered *every end* that had been promised for it, by those who suggested it. Of course the reader is prepared to hear that the old and prominent Institution is persevering in the good works that brought forth a plentiful crop of excellent fruit. But, alas! although the classes improved in their steadiness, excellence, earnestness in study, and in capacity for standing examinations for the honors of the school; although everything worked well, there was a debenture; the classes began to fall off. The reform had worked admirably in qualifying students for the doctorate, and for practice, but the Professors found that their fees were diminishing, and after all the "earnest efforts to improve the system of Medical education in the United States," after proclaiming year by year that the "earnest efforts" were perfectly successful, in the very midst of these elevating and glowing prospects, the Faculty announce that they cannot stand the benefits of the reform any longer. The reform was a good thing to write about, to talk about, to trumpet over the earth, to bring annually before the American Association; it accomplished everything that had been promised for it, it improved students most essentially, made better Doctors, thereby offering something to society, for low per centage in some of the columns of vital statistics, but, notwithstanding all these things, the Faculty had discovered that it diminished, not a fair and reasonable compensation, but to a small extent an extravagant income, if compared with the revenue of a large majority of the Medical Schools of the United States,

and on account of this diminution, the Faculty turn their backs upon that reform, and its good results, which they had lauded annually.

After all the "encouraging results" that have been ardently and perseveringly pressed upon the profession for years, as the fruits of six months terms of lectures; the reader is scarcely prepared to hear that the Faculty have determined to shorten the "good time" to a five months term! The Faculty do not pretend that they have changed their views of the excellence of what they deem the essential results of the long term. But what? Why, the fees are diminishing. At first the classes increased, and then the reform was magnificent; now the classes are lessening, and though the reform was, and is, according to the circular, beneficial to the medical students, to the profession, and to society; yet the Faculty are done with it. The first draught made at extension, caught 509 matriculants, the second 499, third 439, fourth 466, and for 1851–'52, only 410. The class of 1851–'52 opened the eyes of the Faculty to the enormity of reform. The age and prominence of the school induced the display of a love of reform, but "prominence" and long columns of matriculants being matters of some moment to the school; as soon as shadows seem to fall on them, the reform is hurried overboard with hot haste.—What if students were improved, what if the graduates under the long term were better qualified to practice medicine than the graduates under the former system; what if society had its chances of living increased, was the income of the Professors nothing? Could they be expected to benefit the profession and the community on a class of four hundred and ten matriculants? The short class shortened the term, and whether the intention is to continue to shorten the term, if the classes keep up the unpropitious feature of growing less by degrees, we are not informed. It was hoped, by the Faculty, that they would force other schools to follow in their wake, and

that the classes would increase; in both these there was a sad mistake, and the cry is, now, perish reform and all its works.

If it were not for their connection with medical dignity and honor, the efforts of the Faculty to relieve themselves from, what they evidently consider a natural suspicion, that their devotions to Medical Science, and "earnest efforts to improve the system of medical education," are more influenced by the effect of the "efforts" on their income, than by the good results of reform, would be infinitely amusing. The "efforts" were "earnest," indeed, as long as the class increased, and the Faculty admit now, that the results to students, and to society were satisfactory. It is a plain matter of fact, that the Faculty did not feel inclined to submit to any sacrifice, in advancing the interests of the profession. The interests of the Profession and of the Professors, must run *pari passu.*

We do not object to the changes in the system of managing the affairs of the Medical department of the University of Pennsylvania. What we object to is, to see the Faculty strutting through the land with very broad phylacteries, covered with texts of scripture of exceeding devoutness. All men cannot, at will assume airs of disinterestedness for an emergency, and we do not like to see those who can, too lavish of the display. If the circular of the Faculty had fairly stated the fact, that the University was favorable to Medical reform as long as that reform swelled the income of the Professors, and that in spite of the admitted great benefits of the reform, to the profession, and to society, the Faculty deserted it, when their classes began to diminish in number; no one could have objected to the exercise of the right of the Faculty to make such a public announcement. It is true, there might have been cavillers who would have said that a class of four hundred students should be sufficient to gratify any pride of prominence, and any reasonable desires of income. Some might have gone farther, and ex-

pressed the belief that a class of that size should have been sufficient to keep a Medical Faculty from deserting a good cause that was working well. The "prominent school" of the country was scarcely justified in withdrawing from a fair test of a new educational scheme, for no better reason, than because matriculants were diminishing in numbers to some extent. It is a sorry lesson to Medical schools, whose best emoluments are far below the liberal scale that frightened the Faculty of the University of Pennsylvania. These schools find in the indisposition of the Faculty of that Institution, to jeopard in the least degree their emoluments in favor of a reform that they said worked admirably, an excuse for not making any "earnest efforts" to improve themselves, their pupils, or the community. Alas! for science and philanthropy.

The evidence of this position of the Faculty of the University of Pennsylvania, given by their own circular is as clear as the noon-day sun. We cannot believe that they hold the intelligence of Medical readers in as low an estimate as they indicate, in presuming that educated minds were to be deceived by the specious ethics in which the retreat from the good work of reform is attempted to be concealed. The sleeping draught was made so strong, that the narcotic is easily smelled. But the reader may incredulously ask, can these things be? Are you not giving us distorted inferences, instead of facts? Let the doubter read the following extracts from the circular:

"There are two distinct principles upon which a medical school may be conducted; one having reference to the interests of the institution, the medical profession, and the country; the other to the temporary and pecuniary interests of the professors.

If the first of these principles be acted on, the efforts of the school will be so directed as to secure the highest possible qualifications in the graduates. Not only will the amplest opportunities for instruction be afforded, but rules will be established, and, as far as practicable, carried into effect, for securing to the pupil the full advantage of

these opportunities. Certain prerequisites to graduation will be announced, upon the basis of which the public may estimate the value of the degree; and, in order that the public may not be deceived, these prerequisites will be religiously adhered to, unless in a few exceptional cases, in which their strict enforcement might work great individual hardship.

The operations of a school based upon the second principle referred to, that, namely, which regards the temporary and pecuniary interests of the professors as the main object, it is unnecessary to follow out in their details.—Of course, they will be directed to the securing of the greatest practicable amount of income; and all measures will be deemed legitimate which are admissible in the struggles of mercantile competition.

The Faculty of the University need not say that they acknowledge fealty to the first of these principles. Independently of the obligations of common honor and honesty, they consider themselves the depositaries of a great trust, in the proper management of which their personal credit is involved. The school with which they are charged has come to them with a reputation, based upon a long course of honorable and successful effort on the part of their predecessors. They consider it a duty to maintain this reputation by all the means within their reach, even at the risk of loss to themselves individually. They dare not compromise the character of the school by lowering its requisitions and relaxing its rules, though by doing so they might possibly for a time increase the number of matriculants, and of course their pecuniary income. They have felt themselves bound, so far as might be in their power, to keep the business of medical instruction in equal advance with the other great national interests. This could be done only by taking an occasional forward step, so as to test the capabilities of the country. It does not follow that such an experiment must always be successful. Should it on trial prove to have been premature, no pride of consistency should prevent a retracing of the step, until it is ascertained at what point a firm stand may be taken, in accordance with the best interests of the school and the profession. The size of the classes will always enter somewhat into the estimate of the standing of a school; and a certain number of pupils is, for

obvious reasons, essential to its efficient operation, under a system by which such institutions support themselves. While, therefore, aiming at improving the character of the classes, the Faculty must not be indifferent to their numbers. As before stated, they are happy in the conviction that, in the former of these respects, there has been for several years a gradual improvement under the improved arrangements of the school. But it must also be admitted that, under the same arrangements, with the opposing influence of a powerful competition, the numbers have not been of late altogether satisfactorily maintained; and there is some reason to apprehend that, without change, there may be a still further decrease."

That language is certainly plain enough. We feel as though Horace was thinking of something of the kind when he wrote:

I know the right, and I approve it too;
I hate the wrong, and yet the wrong pursue,"

Amidst all the flaunting of the "considerations by which the Faculty were influenced;" is anything more conspicuous in bringing about "certain modifications of the recent course," than that which relates to the "temporary and pecuniary interests of the Professors." While we read the flaming zeal for "the first of the principles," for the management of Medical Schools, we feel that here is the proper spirit. We enjoy the sly thrusts made at those "operations of a school based" on that "which regards the temporary and pecuniary interest of the Professors, as the main object," and all seemed so overflowing with high toned Medical honor and zeal for the interest alike, of the profession and of humanity, that we were about to throw the rest of the circular aside under the conviction that all was right, and rejoice over the glory of the Faculty thus pure and disinterested, when our eyes fell on th following: "a certain number of pupils is, for obvious reasons, essential to *its* [the school] efficient operation under a system by which such Institutions support them-

selves." A "certain number of pupils" has not a very remote bearing on "the temporary and pecuniary interests of the Professors." We began to think that two chancery clerks had been dining together, and mixed a chancery bill and cross bill together, to the utter confusion of equity. Are five hundred pupils any better for the trial of an improved system of medical education, than four hundred? And is it exactly honest to give a class of five hundred all the vast advantages that were claimed for the long session, and withhold these benefits from the class of four hundred, merely because five is something more than four? May not the people, instead of being contented to know that a physician is a graduate of tne University of Pennsylvania, find it to their interest to inquire whether at the time of his graduation, the class consisted of five, or four hundred, and graduate their confidence on the numerals, instead of the Diploma?

But, why insult public intelligence by pretending that the Faculty of the Medical Department of the Unlversity of Pennsylvania, in their abandonment of the long term, were not influenced by private interests? How else is the "character of the school to be compromised, "by a reduction of its class to four hundred or less, while the amount of instruction, and requisites for graduation are increased, and while the smaller classes have exhibited, as the Faculty say is the fact, a decided superiority in diligence and attainments over the preceeding larger classes? If the reform was essential, and demanded sacrifices on the part of Professors; had not the Medical public a right to look *up* to the University of Pennsylvania for a noble example? Who would have expected to find her in such a cause, by looking *down?* Who would have supposed that an Institution of such lofty claims to virtue, could have been turned aside from an "earnest effort to improve the system of Medical education in the United States," by the jingling of monies? Are we to understand that in

all questions for improving Medical teaching, and elevating the profession, the conduct of the Medical Department of the University of Pennsylvania is based entirely on a view of the profit and loss account of the Professors? Are we to understand that on all such subjects, what ever amount of "fine sentiments," which, Joseph Surface like, she may beat up into flummery, it is merely to be poured over bags of dollars, to conceal the true state of case from the eyes of a generous, liberal and philanthropic profession, whose members must be hood-winked for the benefit of Diana of the Ephesians?

The sophistry of the circular, to which we have been referring, is too transparent to deceive many beyond the range of its writers. They possibly worked themselves up to the hope that the tangled web might catch the unwary. Why not make the "modification" without any explanations? Why not make it and repose in grim dignity? Why rouse the public mind by raising the question of disinterestedness? Why dress in the costume of Joseph Surface, and claim to be sailing with John Howard, in the circumnavigation of Humanity?

It seems to us that this is one of those instances of self deception, of which poor human nature has given many examples. The surface of the circular seems to show a deliberate attempt at deception, but we think the Faculty were really hood-winked themselves. This is the charitable construction, and that we prefer. When a man is sensible that appearances are against him, in matters affecting personal motives and actions involving self interest, he cannot be too guarded in giving reasons for his conduct. The liability to self deception is great. While the interested party labors earnestly for excuses and apologies, and rolls forth in majestic streams, swelled beyond their nature, phrases about benevolence, disinterestedness, and philanthropy; a cool, dispassionate observer thinks that the truth would be best expressed by such

terms as avarice, self-interest and cupidity. The veiled prophet of Khorassan was not the last of his race who concealed the most hideous features behind a lustrous veil. Joseph Surface occasionally rises from his grave, and stalks the earth in high-heeled boots, without preliminary "rappings."

The reader may think that the curiosities of the circular are exhausted, but this would be a great mistake. A snarling critic would find a feast in the crumbs that are left, but we shall not indulge our palate. Let us do justice, however, to truth.

While the Faculty of the Medical Department of the University of Pennsylvania were vehemently pressing the superlative advantages of the long term upon the attention of the profession; while they were almost over zealous on the subject, before the American Medical Association, there were many intelligent persons who denied the claim of the University of Pennsylvania to the use of the long term. It was often called a specimen of hocus-pocus. We well remember being present when Professor Miller, of Louisville, delivered a speech before the American Medical Association and stated a series of invulnerable facts that proved the hollowness of the claims to the use of the long term. There was a good deal of fillibustering in an effort to answer Prof. Miller, but his masterly facts were incontrovertable, and the *long term* of the Medical Department of the University of Pennsylvania, received a blow then, from which it never recovered. But at that time the entire existence of Medical education seemed to be staked by the University of Pennsylvania, on the six months term. There was no salvation then, except under six months training, and the "old Philadelphia school" was regularly in the field as the champion of the six months avatar.

We have now two pictures here, and let the reader look first on this picture, then on that. If he will bear in mind

how persistently the Faculty of the Medical Department of the University of Pennsylvania, pressed upon the profession the vast advantages of the *six* months course; how, year after year the most wringing appeals were made to the American Medical Association, to push other schools into the measure, urging that the experience of the Faculty had shown that the essential gains of the *six* months term were increased excellence in the deportment of the pupils, enlarged Medical culture, and a greater ability to pass examinations for Medical honors; and if he has met with many of the numerous discoverers of perpetual motion, and remembers how they glowed over their achievements, such, a reader may enjoy the portrait which the Faculty drew of the greatness, vitality, and essentiality of a *six* months term. That was *the* discovery of the age—the *ne plus ultra* of Medical reform; one for which "prominence" and "old age" could afford to make "earnest efforts in improving the system of Medical education." But, alas! Catalogues began to shrivel a little, and then came "modifications" of the "earnest efforts."

Now, let us look upon the portrait of the "modifications;" a portrait from the same easel, from which the other painting came. The same Faculty that lauded the six months term, and the effects, are now agonising the Medical mind about a *five* months term, and they declare that their Institution neverreally had a six months term, and for fear that some of their credulous friends, who had been convinced of the actual of a six months course might doubt the new phase of affairs, the Faculty enter into an elaborate proof to establish the fact that the Medical Department of the University of Pennsylvania never had such a term. The Faculty *now* insinuate that one week was *wasted* upon introductory lectures; one week of *eight* days was a holiday, a cessation from all labor, and that two weeks, at the last of the session, did

but little service, on account of the want of intercommunion between the Professors, and their restless, worn-out pupils. The proof is decided, and the grand result of the "earnest effort" is, that at least one *sixth* of the long term was devoted to efforts that were neither useful nor ornamental!

The mythology of the Latins taught that Saturn devoured his offspring, but even that wild medley of prurient fancies, did not represent the god as first lauding the altitude and magnitude of his progeny, and then depreciating them before sitting down to his repast. The University of Pennsylvania does not seem to have as voracious an appetite as that of Saturn; she eats rather in the mood that adorned the repast on leeks, which Ancient Pistol accepted at the hands of Fluellen, using the very grace of Pistol:

"I eat, and eat, I swear."

But let us return to the Circular. It says:

"The "Modifications" adopted, will "obviate in some measure, one of the main difficulties on the part of the pupil," without *in any degree derogating from the *great object of improved instruction!*" Indeed, the Faculty go on to present, in a pretty strong light, the advantages of a five months term. And now it appears that there were certain drawbacks to the efficiency of the six months term. "One of these," to quote the language of the report, "is the impossibility of retaining all the students of the first course till the end of the session. Even when the term is short, there is a strong disposition on their part to anticipate the close of the lectures, in their anxiety to return to their home and friends. This disposition is, of course, greater in the prolonged session, and becomes almost irresistable when the example of an early dispersion among the pupils of other schools, in the same neighborhood is placed before them. Young men who see their acquaintances returning home on the first of March, must be gifted with extraordinary self-control, to be willing to linger on till the first of April. *Hence, by*

*The Italics are ours,

*far the larger portion of students not on the list of candidates, and residing at a distance, leave the school as much as two weeks before the *close of the six months session.*"

The report proceeds to mention another objection to the six months term, which is peculiarly applicable to Institutions in which the annual classes are large. We quote as follows:

"Another circumstance qualifying the advantages of the length of terms, has been the necessity of examining the candidates for graduation during the continuance of the lectures, in order that they might not be detained long after the close. But with the prospect of the examinations before them, to the result of which they look with the greatest anxiety, as being likely to influence their whole future life, the young men find it impossible to command a due attention to other objects; and all those objects are consequently neglected, which have no direct bearing on the approaching time. *By the candidates, therefore, as well as by the pupils of the first course, the lectures of the last two weeks of the session are but partially attended.*"

The report adds:

"It has long been the custom, even before the lengthening of the session, to grant the class a recess from Christmas to New Year's inclusive; and the first week of the course has generally been occupied exclusively with the introductory lectures. *Thus two weeks have *been nearly lost to the session.* This has been the case, not only with the University, but also, with other †schools having shorter courses."

The Faculty say, that "from the above remarks it is evident that, though the session in our school has been prolonged to six months, and has fully occupied this period according to the system pursued before the change was made, so that the six months term was really in advance of the former term to the full nominal amount, yet such were the arrangements that a length of time equivalent

*The various Italics are ours.

†This statement may apply to Philadelphia schools, but not to all those in other places.

to about four weeks, if not lost to the business of tuition, was certainly not occupied in the most effective manner."

"Now, by modifying these arrangements, the Faculty hope to be able to accomplish all, in the way of instruction that has been gained by the prolongation of the session, while they spare to the first course student three weeks, and to the candidate one week of the time, and an equivalent proportion of the expense which the six months system cost them."

The Faculty had so repeatedly assured the public that they were "earnest" in six months "efforts," that they no doubt felt that all this elaborate argument was absolutely necessary to exorcise the spirit they had been conjuring for years. Even now, we fear, that in spite of the ample confessions of the Faculty that the six months course was regular humbugging, there are many who were won to the former faith by the asseverations of the Faculty, who may cling to their old faith. Indeed, we think that none could be fairly charged with the commission of an outrage, who might charge that the Faculty of the "Old Pennsylvania school" did not write the circular. They might fortify themselves by asserting that there is not a fact, nor an objection as to the lapses of the *long term*, that was not known to the Faculty, when year by year they were proclaiming that they were honestly engaged in the "earnest effort" of the full six months term. The sturdy doubter might add, that surely that Faculty would not stun the senses of the profession by repeatedly asserting the immense results of six months teaching, and now proclaim that their students never really had more than a five months course! Perhaps some one might add, that if the present statements of the circular are true, some awful monstrosities have been palmed upon the profession, from the same quarter. Does that Faculty hope that the profession has taken chloroform, in order that these unconscionable things may be slipped in during a state of unconsciousness?

The "modifications" referred to, consist in giving the introductories in two or three days instead of appropriating a week to this purpose; limiting the recess or hollidays to Christmas and New Year's day; convening the

session on the second, instead of the first Monday in October, and terminating it in the middle in place of the end of March, and postponing the examinations until after the lectures have closed. A preliminary term of two weeks is also to be given, "devoted to subjects which, though practically useful to the student; cannot, for want of time, be treated in sufficient detail in the ordinary course." In the speech of Prof. Miller, to which we have alluded, he gave specimens of these "practically useful" preliminary lectures. His hearers will not soon forget the account he gave of those lectures; lectures too, which he heard delivered. We forbear detail. Let us push forward.

The arguments thus adduced in favor of the five months course are cogent; but why are they urged now? Why have they lain dormant for the past three years? If the term was but of *five* months length, why so often and so publicly assert that it was full *six* months, and was working miracles among students by being a *six* months course? Was no explanation due to the public on this point? How can any body of men parade such glaring antagonism before intelligent people? The Faculty are now feverish with the idea that the falling off of their classes is due to the long term. After a while they may begin a new series of cavortings that may lay in the shade even those we have just been gazing at in wonder. Time may show the Faculty that even a five months term cannot open the tomb. The age of centralism in medical education, is past, and the prestige of an ancient institution cannot recall it from its grave. The University has now leaped into the territory upon which she recently made war, and has made the duration of her sessions conform to that of other schools. Time and honest catalogues will tell the rest of the story.

We have been thus plain in our remarks, because we felt that the occasion demanded plainness of speech. We felt too, that this was due from us, because no improper motive can be justly suspected in us. We neither belong to, nor have any, the least connection with any Medical School. For this reason our criticism cannot be open to the suspicion which, the circular palpably reveals as the cause of the "modifications" of the lecture term of the Medical Department of the University of Pennsylvania—of self-interest B.

SELECTIONS

FROM

FOREIGN AND HOME MEDICAL JOURNALS.

Fellis Bovini, as a Medicine. By A. Cummings, M. D.—The bile from the gall of the ox has long been known to possess medicinal properties, and to some extent it has been used by the profession as a remedial agent, but I believe it has never gained that confidence among practitioners of which its real value renders it worthy. I have used it somewhat extensively in my practice for a few years past, and in this article I design only to give the result of my own experience with it, and the conclusions to which I have arrived in relation to its real and comparative value. Before proceeding, however, I would remark that the form in which I have almost immediately exhibtied it, is that of pills, made of the inspissated gall, rolled in flour, magnesia, Pulv. Glychyrriza, or some other fine powder, to render them of a suitable consistency. The gall may be evaporated in shallow basins, in an oven, or in the sun, until it becomes sufficiently firm to form into pills as above. This, in my opinion, is far the best method. It may be given in its liquid form, but it is less agreeable to the patient, and if not mixed with proof spirits will soon become unfit for use. The medical properties of this agent, so far as my observation goes, are *laxative, al-*

terative, and slightly tonic. Its most valuable agency is exerted upon the stomach and liver. It seems to combine, in a remarkable degree, the three properties named above, and for many diseases to which the chylopoietic viscera are liable, I have found it a most excellent and valuable remedy. I proceed now to notice its application as a remedial agent in disease.

1st. *Obstinate Constipation.*—I am well aware that this is much oftener a symptom of disease, than disease *per se;* but I notice it in this place in order to speak of it in that form so common to those engaged in sedentary and confined occupations, and especially of females in large cities, amongst whom, for want of proper exercise and care, constipation is so prevalent and detrimental. In cases of this description, I have seldom, if ever, been disappointed of obtaining relief by the use of the agent under consideration. The hardened, compact, clay-colored fæces so common in cases where there is more or less obstruction of the liver, are, by the use of the gall, broken down in the intestines, and rendered so friable as to be easily discharged. This result is procured, not only by the combination of the gall with the hardened concretions, rendering them soft and unirritating, but also by removing the obstruction, and permitting the natural flow of bile from the gall-bladder into the intestines. Thus it answers, in this respect, a two-fold purpose. Any one who doubts the efficacy of this remedy, by pouring a few drops of fresh gall upon hard clay-colored fæces, and observing how soon the mass becomes liquid, cannot fail to be convinced. It imparts a healthy tone to the bowels, and promotes the natural secretions which may become impaired.

2d. In *bilious diseases,* arising from a torpid action of the hepatic function, the gall is an excellent remedy. It seems to act as a stimulant to the liver, and promote the secretion of the bile, and also to cause it to flow freely into the bowels, and thus accomplish its normal functions in the animal economy.

3d. In *Jaundice,* you will find that the exhibition of gall, if continued sufficiently long, even in small doses, will not fail to accomplish a desirable and satisfactory purpose. I could relate the history of many cases in my own practice, in which there was every symptom of this

disease, where the gall has acted in the most satisfactory manner. As a stimulant to the liver, I generally prefer it to the blue pill, or mercury in any form, though there may be chronic diseases in which the mercury or some other alterative would be preferable. Whoever, at least, will thoroughly test the powers of this agent, I am confident, will find that I have not exaggerated its value. At least I am willing to abide by the judgment of others who may test it, as to the truth of my assertions. It may be necessary to continue the exhibition of this remedy for some length of time in several cases of jaundice, but it is perfectly harmless, and moreover it may be used in those cases in which mercury cannot be administered, on account of the prejudice of patients or their friends against it, or of any idiosyncracy of constitution, where its use may be interdicted. At least it is a most valuable auxiliary.

4th. In *Dyspepsia*, also, I have in many cases seen the most gratifying results from the use of this remedy. It seems to impart a good tone to the stomach, and by its laxative effects upon the bowels, as well as by its soothing the irritated mucous surfaces of the stomach, proves, in my hands, at least, an excellent remedy. It leaves the influence of a mild tonic bitter on the stomach, not sufficient, however, to produce pain; and its laxative effects in dyspepsia cannot be but beneficial, for, in most cases in this disease, the bowels are torpid, and not unfrequently obstinately constipated. As a remedy also collaterally,

5th. In *Hemorrhoids and Prolapsus Ani*, the gall is justly entitled to our consideration and confidence. If it has no direct or specific influence in removing these forms of disease, it is at least one of the best laxatives in the general torpor of the bowels which accompanies them, since it not only evacuates, but soothes the bowels, and does not produce the irritation in piles and prolapsus ani that most other articles of the class do. But I am also inclined, from my experience with the article, to believe that it exerts a very favorable influence, at least, in the cure of these troublesome and painful affections. At least it justly merits a fair trial.

6th. In *Bilious and Intermittent Fevers* the gall cannot but exert a favorable influence, since its office is not only

to act as an alterative, and rouse the liver to its wonted action, but also to carry off from the bowels the superabundant bile, and to give tone to the chylopoietic system. In a word, in all those forms of disease, (and they are many,) arising from torpor of the hepatic system, I believe that there are few medicines that will give equal satisfaction with the one under consideration; and it is certain that no remedy is more safe in its administration and effects.

One more tormenting and dreaded effect arising from bilious derangement, in which the gall acts in a very favorable manner, I had almost forgotten to mention.

7th. *Sick Headache*, so called, though not strictly speaking a disease, is a sympathetic, symptomatic affection, of very frequent occurrence, and always excrudiating, and dreaded by those who are subject to it periodically or occasionally. As it arises from a bilious state of the stomach, the gall, given in small doses, and as frequently as is necessary, seldom fails to mitigate the symptoms, or entirely to relieve, or prevent its accession. It should be given in periodical cases for a season of at least a day or two before the anticipated attack.

8th. In *Typhus and Typhoid Fevers* it is an excellent laxative, where strong cathartics are not required, and will be found worthy of confidence whenever a remedy of the class seems to be indicated. Also the low forms of

9th. *Nervous and Continued Fevers*, no better laxative, in my judgment, can be found, since in those cases strong cathartics are almost invariably contra-indicated. But I need not particularize further, since I believe enough has been said to give my ideas in relation to the class of cases in which this remedy is indicated; and as I cannot expect to gain the confidence of practitioners without their first giving the article in question a fair and impartial trial, I have perhaps already written too much. I am confident, however, that those who make a fair trial of it will not accuse me of exaggeration, for I have endeavored candidly to give the value of the article as it has proved itself in my own practice, and not from theory deduced from the natural properties of the medicine. I have said it is necessary, not unfrequently, to continue the medicine for some length of time, in obstinate cases, especially. But

it is harmless and safe, and will act well in any constitution, and is contra-indicated by no form of idiosyncracy. The dose of the inspissated gall in the form of pills, or otherwise, is from five to ten grains or more, repeated every two or three hours for a cathartic, and less for a mere laxative effect. It may be given in sufficient doses, at any time, with perfect safety to adults or children.—*Boston Med. and Surg. Jour.*

The Radical Cure of Reducible Hernia, by Injection. By John Watson, M. D.—I do not propose, just now, to go into the whole merits, or demerits of this operation, a task which has been assigned to other hands, and which has already been in some degree achieved, by one of the committees of the American Medical Association (see their report in the Transactions for the session of May last.) My only object is to give the details of a single case treated in this way, in connection with such hearsay information as I have been able to collect concerning the operation in other quarters.

The procedure now under consideration, if I am not mistaken, was first brought into notice through an irregular channel, by a certain Industrialist, of New England. But the first notice I remember to have seen of it, was in Professor Pancoast's *Operative Surgery.* In July, 1848, my attention was for a second time called to this subject, by a gentleman under my care for the treatment of varicocele, who, a year or more previously, had undergone an operation for the cure of a reducible hernia, by what he described as a trifling process, and with complete success. He spoke of it as a simple puncture, which subjected him to very little uneasiness; and assured me that he was cognizant to the cure or relief of other individuals, who had been treated like himself, by a practioner of Boston. I subsequently ascertained that the instrument with which this individual operates, had been prepared by a cutler of this city; but on inquiry I found that it had been patented by the operator, and was, consequently, to be used only by himself. As I had made inquiry for it, with the view of employing it on a case then in hand,

could not but feel indignant that any practitioner claiming to belong to the regular profession, should have thus prostituted his noble calling to mercenary ends; but believing that no special form of patent instrument is essential for the making of a puncture, or for introducing an irritating fluid beneath the integuments through this, I attempted in my own way to get along without it, as in the following case:

Joseph A. Seavell, of Ohio, seaman, aged 31, was admitted into the New York hospital, Nov. 24, 1851, with a large inguinal hernia, occupying the left side of the scrotum, which had been then protruding for several hours, and had resisted several well-directed efforts for reduction. The patient for the last four years had been occasionly troubled by the protrusion, but had never before been baffled in his efforts to reduce it; and by the use of a truss he had been able to follow his regular occupation. With some little trouble the tumor was reduced by taxis, soon after his admission; and on the 29th of November, having explained my object to the patient, and obtained his consent, I attempted to effect a radical cure of the hernia.

While the patient was lying on his back, with his scrotum and left spermatic cord drawn slightly towards the right side, and with the integuments over the left external abdominal ring slightly on the stretch, I introduced the point of a delicate bistoury through the integuments, directly down to the crest of the os pubis, the point of the instrument touching, without dividing, the lower termination of Poupart's ligament, and made to work freely in the loose tissue immediately in front of the ring, but without wounding the spermatic cord. Having made the puncture, and withdrawn the bistoury, the nozzle of a small syringe charged with tincture of cantharides, was introduced through the wound, and about a drachm of this fluid was injected into the bottom of the cut, the hand of an assistant, in the meanwhile, resting firmly over the inguinal canal to prevent any portion of the injected fluid from entering this, or passing through the sac into the abdomen.

The whole procedure was the work of a few seconds, and gave the patient little or no uneasiness. I next applied a compress and spica bandage, to keep the parietes

of the inguinal canal in close apposition, and administered an anodyne, keeping the patient on his back, with directions to apply an evaporating lotion, should severe inflammatory symptoms supervene.

In a few minutes after the operation, he began to speak of pain from the injection. The sore became more troublesome, and extended for several inches in every direction, but was severest along the ascending track of the spermatic cord. He slept but little during the following night; but next morning the pain had subsided, a slight soreness, only, remaining in the part. The patient was at the same time suffering from chancres. I made the treatment of these the pretext for keeping him on his back with the compress and bandage applied as above, for several days. He spoke of no uneasiness from the operation after the second day. On the 12th of December he was walking about without his truss, and with no apparent tendency to a recurrence of the hernial protrusion. On the following day, being desirous to join his vessel which was about to sail for South America, he requested his discharge, promising to write to me, and report the further progress of his case, should the swelling reappear—and, if possible, to report in person, at the close of his voyage. But, as yet, I have not heard of him.

The operation in this instance had evidently a beneficial effect, and I am not certain that it may not have effected a permanent cure. I am not disposed to believe that any portion of the injected fluid entered the hernial sac; but by exciting inflammation around this within the column of the external ring, and the subsequent condensation of tissues, which is so apt to follow acute inflammation, we can readily imagine that this procedure may, now and then, effect an object we have hitherto sought in vain to effect by other and severer measures.—*N. Y. Medical Times.*

Paste made from Pumpkin Seeds in the Treatment of Tænia. Translated from the French, by S. W. Butler, M. D.—We have often repeated that our indigenous materia medica is not as meagre as is made to appear, and if we employed in its study the care which we bestow upon

that which commerce brings us at great expense, this truth would not need to be repeated. Here is an example. It is more than thirty years since the *Journal Universel des Sciences Médicales* described the good effects which Dr. Mongeny had obtained in cases of tænia with a paste composed of ninety grammes (about 3½ ozs.) of fresh pumpkin, and double that quantity of honey, given in three doses, an hour apart. By this means, this physician had constantly succeeded in dislodging the tænia in the course of six or seven hours, even, in many cases after all the remedies then known had failed.

This result was sufficiently important, and the innocuity of the medicine incontestable enough to have provoked an extended trial, but observations upon it have only been made by the practitioners of Bordeaux. In 1845, Dr. Brunet pointed out to his colleagues of the Medical Society, the remarkable results which he had obtained by the use of pumpkin seeds, and since that time many of his brethren have cited numerous cases of expulsion of tænia by the employment of this remedy.

At Bordeaux, the paste is prepared with forty-five grammes (about 1¾ oz.) of the hulled seeds of the large pumpkin (*cucurbita maxima*) with the same quantity of sugar.

Here is another proof of the success of this remedy. A physician suffered almost constantly with pains in the lumbar region, and with general lassitude; the least labor fatigued him greatly. This state had discouraged him, and made him fear an affection of the nervous system.—Having perceived that he discharged living white worms, resembling pieces of flattened dog's grass, (*chiendent aplati,*) of the length of an inch, or nearly, and although he distinguished in them a perforation, and not the slightest trace of articulation, he thought himself affected with tænia. In accordance with the advice of M. Saramea, he took, at eleven o'clock at night, thirty grammes (about an ounce) of pumpkin seeds, mixed with one-third the quantity of sugar. The next morning—twelve hours after—having taken a simple injection, this physician passed seven metres (a little more than seven yards) of tape-worm. We cite this case simply to set forth the efficacy of the means employed; and we must here mention that, in the discussion brought forth by this fact, Dr.

Brunet said he had employed this remedy twenty-five or thirty times with success since his first communication to the Society. M. Saramea has also succeeded in a great number of cases, but he has been obliged sometimes to employ the remedy a second or even a third time. This physician cites two cases in which the root of the pomegranate, and even kousso, had failed. It is not necessary to believe, however, [in regard to the remedy in question,] that no instance of failure may occur. M. Brunet and M. Costes have pointed some out, and they have asked, with just reason, if these various results may not be consequent upon the species of tænia treated. This remedy has seemed sufficiently efficacious to warrant the Society in entertaining the proposition of one of its members, to address a note to the National Academy of Medicine, with the request to order its insertion in its proceedings, and ultimately in the *Codex.*

This communication will, we doubt not, be received favorably by the Academy, and we would hold it up as an example worthy of imitation—as occasion offers—by other societies!—*N. J. Med. Reporter.*

Treatment of Hooping Cough.—In Hooping Cough, Dr. Madison, of Virginia, recommends a blister to the nucha; Dr. Golding Bird uses *conium*, in the following form: R Ext. Conii, grs. xij; Alum, grs. xxv.; syrup papaver, ʒiij; aq. fœniculi ℥iij; M. A desert-spoonful every four or six hours. Dr. Spengler, in a French journal, recommends *conia* in doses of one-fortieth of a grain for children three or four months old, and one-tenth of a grain for children of as many years. He has employed it in several cases successfully.

Strangulated Hernia in a Child five months old; Operation; recovery. Under the care of Mr. Erichsen.—The following particulars were obtained from the notes of Mr. Turle, Mr. Erichsen's house-surgeon:

Arthur C——, aged five months and a half, was admitted April 12, 1852, under the care of Mr. Erichsen. It appears that when the boy was three weeks old, a hernial tumor appeared in the right groin; it could be easily reduced, and would only come down occasionally—viz: about once a week. The tumor then used to remain apparent for several hours, and afterwards ascend spontaneously into the abdominal cavity. When the child was four months old a truss was applied, but the apparatus proved ineffectual, as the tumor appeared again on the following day by the side of the pad. The truss seemed also to cause considerable pain, and the mother, therefore, brought the child to the hospital.

The little patient was found, on examination, to be affected with oblique inguinal hernia of the right side; the intestine had descended into the scrotum, and was greatly distending it. There were, however, no symptoms of strangulation at the time; some attempts were made to reduce the tumor, but these having failed, the mother was desired to leave off the truss and to bring the child immediately she perceived any unpleasant symptoms.

Six weeks after this (April 12, 1852) the mother applied to Mr. Erichsen again, as alarming signs of strangulation had manifested themselves. The tumor, which had remained unreduced, was now large and tense, its neck appearing to be tightly constricted by the external abdominal ring. In following the intestine down into the scrotum, it was remarked that the latter was not only distended by displaced intestine, but likewise by fluid secreted in the cavity of the tunica vaginalis, so that the child was suffering both from strangulated inguinal hernia and hydrocele. Constipation and vomiting had been existing for some time, the surface was cold, the face pale and drawn, and the patient evidently in a very precarious state.

The mother, having been told of the dangerous condition of her child, readily consented to the performance of the operation, and Mr. Erichsen proceeded, as soon as the child was narcotized by chloroform. The integuments

were divided over the neck of the tumor, and the several layers of cellular tissue and faciæ having been carefully slit open, the sac was fairly exposed. The constriction was now found to be exerted by the external abdominal ring, and a few transverse fibres of the cremaster muscle; a curved director was passed beneath these parts, the strangulating structures were divided by an incision directly upwards, and the protruded intestine returned with facility without the peritoneal sac having been opened. The edges of the wound were approximated with sutures, and the whole secured by a compress and a double-headed roller.

Three hours after the operation a copious liquid motion was passed, the child being perfectly quiet and composed. On the following day he was in a very satisfactory state, and the wound looked very healthy. From this time the little patient progressed most favorably; he had no bad symptoms whatever, became cheerful, and improved much in appearance. The bowels were open daily, and the child left the hospital, the wound being quite cicatrized, eighteen days after admission.

It will be perceived that the taxis was not tried at all in this case, when the symptoms were fully established; and that this cautious conduct has a marked and beneficial influence on the issue, is clearly seen by the success here obtained. We cannot help noticing, in reviewing this case, the baneful effects of the common spring truss in infants; nor can it be otherwise, for it is next to impossible that the pad should exactly compress the ring with infants at the breast. It is equally difficult to regulate the spring in such a manner as to prevent the truss from slipping, without using an amount of pressure which must of necessity be hurtful to the child. Nothing will answer in such cases but elastic belts, which yield without becoming displaced when the infant cries; the belt being supplied with an air-pad, as suggested by M. Bourjeaurd, which shall be gently lodged upon the ring, and be made softer or harder, by means of the stop-cock, as occasion may require.—*London Lancet.*

Prevention of Salivation. By George Stearns, M. D.

I wish to communicate a fact to you that has recently fal-

len under my observation, which may be of some interest to the profession generally. All physicians are aware of the salivating effects of calomel, and of the inconvenience that arises from sore mouths and other irritating complaints that affect the patients.

I have had several persons under my care to whom I have been obliged to administer calomel, which I have mixed with supercarbonate of soda, in the proportion of about twice the amount in weight of soda. To one patient in particular, whom I have attended for about ten weeks, I have given three grains of calomel with six grains of soda daily for five weeks, besides administering it frequently during the rest of the time. As yet he has not suffered at all from the salivating effect of the calomel, which has nevertheless been very beneficial to him. It is possible that these were all persons not susceptible to salivation? Or is the absence of salivation to be attributed to the supercarbonate of soda?—*Boston Medical Journal.*

A Case of Poisoning with Oil of Tansy—Death at the end of three hours and a half—Quantity of the Drug taken about ℥j *and* ʒiii. By John C. Dalton, Jr., M. D. (Read to the Boston Society for Medical Observation, June 2, 1851.)—E. S., a fine, healthy-looking girl, about twenty-one years of age, died at the house of Mr. A., in Boston, on Wednesday, the 7th of May, 1851. She had been employed in Mr. A.'s family as a seamstress since the previous winter, living in the house during the week, but going away on Saturdays to a cousin's in Pleasant street, and returning to Mr. A.' son Monday morning. She had been, for some months, receiving the attentions of a young man who was reputed to be engaged to her. None of her friends, however, suspected anything to be wrong with her until Monday, May 5, when her cousin, with whom she had been spending Sunday, as usual, perceived the odor of tansy in the room which she had occupied; whereupon it occurred to her that the girl might have become pregnant, and used the drug for the purpose of producing abortion.

On Tuesday, she was engaged in her ordinary employment, and dined heartily a little after five o'clock in the

afternoon. She went up stairs to her room about half-past nine o'clock. The cook, who occupied a room above, went up with her and stopped in her room, conversing for some fifteen minutes. The girl's manner was perfectly natural and cheerful, as it had been throughout the day. About a quarter before ten o'clock the cook left her preparing for bed, and went up to her own room.

Nothing more was heard from her till about eleven, when Mr. and Mrs. A., who were sitting in the basement-room, heard a scream, which they supposed to come from one of the children. Mrs. A. went immediately up stairs, and on entering Miss S.'s room, found her on the floor, by the side of the bed, insensible and in violent convulsions. She had evidently fallen out of bed, as she was undressed, and the bedclothes were disturbed, and had been partially dragged on the floor with her. Dr. Morrill was immediately sent for, and arrived in about ten minutes. He sent also for me, and I arrived at the house at half-past eleven o'clock.

The girl was then lying on her back by the side of her bed, and presented the following appearances: Total unconsciousness; cheeks flushed; of a bright, red color; eyes open and very brilliant; pupils of equal size; widely dilated and immovable; sclerotics injected; skin warm, not remarkable as to moisture; respiration hurried, labored, stertorous, and obstructed by an abundance of frothy mucus, which filled the air-passages, and was blown from between the lips in expiration; the breath had a strong odor of tansy, as had been already observed by Dr. Morrill; pulse quite full, forcible, 128; at intervals of five to ten minutes the body was convulsed by strong spasms, in which the head was thrown back, the respiration suspended, the arms raised and kept rigidly extended, and the fingers contracted. After this state of rigidity had continued for about half a minute, it was usually succeeded by a tremulous motion, often sufficient to shake the room, together with very faint and imperfect attempts at inspiration. The whole interval from the commencement of the convulsion to the first full inspiration, varied from a minute to a minute and a-half. Occasionally, the tongue was wounded by the teeth, and the saliva slightly tinged with blood. Immediately after a convulsion the countenance was very pallid and livid, from the

suspension of respiration, and the pulse exceedingly reduced in strength and frequency. The pulse and color then gradually returned until the occurrence of the next spasm. It was very common, a few seconds after the termination of a convulsion, for the head to be drawn slowly backward, and the eyelids, at the same time, stretched wide open. In the intervals of the convulsions the limbs were mostly relaxed, but the jaws remained clenched.

A vein was immediately opened in the right arm, and about Oij of blood taken away. After this, the pulse became much softer, and the face lost its bright color. There was, however, no change in the condition of the pupils, nor return of consciousness, nor other improvement in the appearance of the patient. It being impossible to get anything down the throat, two injections of an ounce of wine of antimony, with about ʒss of powdered ipecac., were thrown up the rectum at intervals of about half an hour, but produced no apparent effect.

On searching the room, a ℥ij phial was found in the pocket of the girl's dress, wrapped in a piece of paper, labelled "Oil of Tansy," and marked with the name and address of an apothecary in Pleasant street. The phial contained ʒv of the oil of tansy of the ordinary purity.—A mug was also found from which she had apparently drunk the oil, mixed with water, as it smelt very strongly of the drug, and still had a drop or two of it at the bottom.

The condition of the patient continued much the same for about an hour. The convulsions, however, gradually became less protracted, and the failure of the pulse after each attack, more complete, at the same time that it recovered strength less perfectly in the intervals. The countenance also became somewhat sunken and the temperature of the skin reduced. About 1 o'clock, six leeches were applied to the forehead and temples, and sinapisms put on the calves of the legs. The leech-bites bled freely.

Toward two o'clock the alteration for the worse became quite rapid. Pulse 124 and feeble; respiration 36, and attended with less muscular effort than at first; the left cornea was glazed, but the right continued brilliant; a little inward strabismus of the right eye, and the mouth

and nose drawn a little to the right side. Occasionally a slow, lateral, rolling motion of the eyeballs. At five minutes past two she had the last convulsion, which was much less violent than the earlier ones, and lasted only half a minute. There was no recovery of the pulse after this attack, and she died at a quarter past 2 o'clock, A. M.

The present case is another instance of the extreme violence to which the system may be subjected even in the early months of pregnancy, without inducing abortion. Though all the muscles, both of the body and limbs, were for three hours and a quarter subjected to a succession of the most violent contractions, there was no sign of abortion, and after death the ovum was found in the uterus entirely undisturbed. In Dr. Hildreth's case, also, pregnancy existed but a few weeks advanced, and the drug was undoubtedly taken for the purpose of producing abortion, but nothing of the kind took place. The general symptoms in that case were similar to those described in the foregoing, the most remarkable difference being the more gradual loss of consciousness, and the more rapid death after a much smaller dose.—*American Jour. Med. Sciences.*

On the influence of Poisons upon Animal Heat as a Cause of Death. By E. Brown Séquard, M. D., of Paris. Prévost and Chossat, and after them, M. Magendie, have ascertained that death occurs quickly in mammals when their temperature is notably diminished. My experiments confirm the correctness of that statement. The diminution of animal heat in mammals is so dangerous, that in one case I have seen death take place in a rabbit after a diminution of only 22° F. (12° Cs.) I have never observed any animal continuing to live when I had diminished its temperature more than 44° F. (24°.5 Cs.) I have found the law established by Chossat perfectly correct, according to which the diminution of animal heat necessary for killing is less and less, in proportion to the rapidity with which that diminution takes place.

It is very probable that in all the cases where, in consequence either of disease, or of a wound, or of poison, the temperature of man is diminished many degrees, his

life is in danger from the very fact of that diminution. It is thus in cholera, in sclerema, in certain cases of palsy, in cases of great disturbance of the respiration, in fractures and luxations of the vertebral column, in the cervical, and even in the dorsal regions, in cases of profuse hæmorrhage, and in many cases of poisoning when death is not rapidly produced.

It has been long known that temperature is diminished in poisoned persons; and there are but few cases of poisoning on record in which it is not said that the patient was cold. Chossat has found that a dog, into whose veins he had injected opium, had its temperature diminished from 105° to 62.6° F. (40°.3 to 17° Cs.,) 22 hours after the injection. Brodie has found that many poisons act upon animal heat so as to diminish it considerably. Demarquay and Dumeril, junior, and later, these two experimenters, joined with Lecointre, have found the same thing as Brodie in many toxic agents. I have made very numerous experiments on this subject, and some of their results have been published before the last papers of Demarquay, Dumeril and Lecointre.*

I have stated that many poisons, either injected in the veins or absorbed by the vessels of the skin or of the digestive canal, may diminish sufficiently the temperature of Guinea pigs and rabbits to produce death. This occurs when the dose of the poison is not large enough to kill in less than four or five hours. These poisons may kill only by their action upon animal heat. It may be so with opium, cyanhydric acid, the cyanide of mercury, hyoscyamus, digitalis, belladonna, tobacco, euphorbia, camphor, alcohol, acetic, oxalic, sulphuric, azotic, chlorohydric acids much diluted, and some oxalates.

Of course the action of these poisons is the greater, the colder the atmosphere; but it is not always immediately so, and instead of diminishing the animal heat, many may increase it for a time, especially when the temperature of the atmosphere is elevated.

I have discovered that a dose of one of these poisons, sufficient to kill an animal, when there is no obstacle to the diminution of its temperature, may be unable to destroy life when the temperature of the animal is main-

* See Gaz. Med. de Paris, 1849, t. iv., p. 644.

tained by artificial means to its normal degree, or not far from it. My experiments have been conducted as follows: —

Equal doses of poison were given, simultaneously, to two animals, as much alike one another as possible. One of them was left in a room at a temperature of from 46 to 50° F., (8 to 10° Cs.,) and the other was kept not far from a chimney, in a place where the air was at from 75 to 86° F., (24 to 30° Cs.) The first was dead after a certain number of hours, or sometimes one or two days, having its temperature much diminished. The other, on the contrary, had no perceptible diminution of its temperature, and was generally cured very soon. Therefore, when taken in certain doses, many poisons may kill only by their influence on animal heat, physicians, in cases of poisoning, should try as much to prevent the diminution of temperature, as to expel the poison or to act against it by an antidote, by pulmonary insufflation, or otherwise.

In experiments which I have made lately on the action of very pure digitaline, that had been prepared by M. Quévenne, the celebrated chemist, who has made such interesting and accurate researches on digitalis and the substances of which it is composed, I have found that this poison may also diminish temperature. I believe it is easy to explain the contradiction existing between Traube and Stannius, as regards the influence of digitalis on animal heat. When the atmosphere, in which the animal is, is cold, then its temperature may be diminished by digitalis or digitaline, but when it is warm the diminution does not take place, or it is very small. But, of course, if the dose is sufficient to kill very quickly, then it is indifferent whether the atmosphere is cold or not, because there may be not time enough for the diminution of the temperature of the animal.

I have to relate another fact which, I believe, ought to be considered as analogous to the preceding. It is that kind of poisoning which occurs when a layer of oil, of varnish or of gelatin, put on the skin of a warm-blooded animal. Death, then, is very probably produced by a substance unknown until now, and which is secreted by the skin. The layer of oil, varnish, or gelatin preventing that secretion taking place, that unknown substance

becomes accumulated in the blood, and then are produced the phenomena so well studied by MM. Fourcault, Becquerel, Breschet, and Magendie. I have found that in such a case the animals may live, if the atmosphere in which they are kept is at a temperature inferior to 79 or 80° F., (26 or 27° Cs.) In these circumstances their temperature is not sensibly diminished, while it diminishes much when the atmosphere is cold. Therefore it is especially by their loss of warmth that animals are killed, when their body has been entirely covered with oil, varnish, or gelatin.—*Ib.*

On the Normal Degree of the Temperature of Man. By E. Brown Séquard, M. D., of Paris. The degree of animal heat, in the human species, is stated as being between 98.5° and 100° F., (37 and 38° Cs.) I intend to prove that it is higher.

It is impossible to take directly the temperature of most of the internal parts of the body in man; therefore many physiologists, in order to discover the temperature of these parts, have argued as follows: According to J. Hunter, the temperature of the rectum in animals is the same as that of the right ventricle of the heart; hence it has been concluded that as the temperature of the rectum, in man—still according to Hunter—is 98°.42 F., (36°.9 Cs., the temperature of the internal parts of the body ought to be between 98 and 99° F., (36°.67 and 37°.22 Cs.)

Some other physiologists, noting the temperature of the mouth under the tongue, and supposing that this temperature is nearly the same as that of the internal parts of the body, have concluded that the temperature of man is between 99 and 100° F., (37°.2 and 37°.8 Cs.)

We will prove that these deductions are not right:

Firstly, the degree of the temperature of the rectum is not, as Hunter says, 98°.4. It may be so in debilitated men, but in healthy persons it is more elevated. It is between 100 and 102° F., (37.7 and 38°.89 Cs.,) according to my own researches and to those of Berger and Maunoir. Besides, many experiments on dogs, rabbits and Guinea pigs, have shown satisfactorily to myself that

the temperature of the rectum is not equal to that of the right ventricle. This last organ is from 1 to 3° F., (0.55 to 1.7° Cs.,) higher than the rectum.

The temperature of the rectum, in man, being between 100 and 102° F., (37°.7 and 38°.8 Cs.,) and the temperature of the right ventricle, in man, being from 1 to 30° F., (0.56 to 1.7 Cs.,) higher than that of the rectum, if we suppose the same difference existing in man as in mammals, it follows that the temperature of the right ventricle of the heart in man ought to be between 101 and 105° F., (38°.33 and 40.56 Cs.)

But a closer approximation of the exact temperature of the internal parts of the body, in man, may be obtained by taking the temperature of an organ situated deeper than the rectum, and, consequently, less exposed to the influence of the atmosphere. Such is the case in the bladder. I have observed its temperature by taking that of the urine at the moment of its emission, and before any sensible change had occurred in it. The urine was directly received in a vase dipped into a large quantity of water at 98° F., (36°.7 Cs.) Thus I have ascertained that the mean temperature of the urine, in man, is 102°.6 F., (39°.2 Cs.)

Now if we take notice of this well known fact, that the temperature of the lower part of the abdomen is less elevated than the upper part, we are authorized to believe that the temperature of the central parts of the body, in man, is very near 103° F., (39°.5 Cs.)

My experiments on the temperature of urine were made on ten strong sailors, in the spring, on the Atlantic Ocean, between the 43d and 45th deg. of north latitude. The lowest degree of the temperature of urine which I have observed, was 100°.9 F., (38°.3 Cs.;) the highest was 103°.2 F., (39°.56 Cs.) My own urine, examined more than thirty times, in the most varied conditions, has been nearly always at the same temperature; the variations have been only between 102° and 102°.8 F., (38°.89 and 39°33 Cs.) The ordinary degree is 102°.5 F., (39°.17 Cs.)

Long before my researches, the temperature of urine, in the human species, had been taken by some observers, but in general without sufficient care. The temperature of the urine is 94°.25, according to Braun; 98°.9, according to De Lisle; and 103°, according to Hales. Recently

Berger has taken the temperature of urine in the bladder in five women. He has found it equal to 101°.48 F., (38°.6 Cs.)

As the temperature of woman is a little inferior to that of man, the result obtained by Berger is in accordance with mine.

If we take the average between the temperature of the urine of man as I have found it, and that of woman as Berger has found it, we have a number very near 102° F., (38°.9 Cs.)

From these facts we draw the conclusion that the temperature of the thoracic and abdominal viscera, in the human species and in both sexes, is between 102 and 103° F., (38°.89 and 39°.44 Cs.,) *i. e.*, some degrees higher than it is generally admitted.—*Ib.*

On the Influence exerted upon the General Temperature of the Body by a change in the Temperature of one of the Extremities. By E. Brown Sequard, M. D., of Paris. The following sentence is given, as an axiom, by Dr. W. F. Edwards: "We cannot either raise or lower the temperature of any one part of the body, without all the other parts of the frame being affected and suffering a corresponding rise or fall in temperature, more or less, according to circumstances."*

Expressed in such terms, we accept this law as perfectly true. But the author elsewhere gives an extension to this law, which we will prove to be incorrect. No doubt when the temperature of the blood, coming to the heart from a remote part of the body, has been modified in that part, the thoracic viscera ought to have their temperature modified; but to what extent? It is on this very important point that we do not agree with Doctor Edwards. He was of opinion that the influence exerted by a small part, on all the other parts of the body, was considerable. He says that the chilling of a single part, such as the hand or the foot, may cause a loss of temperature in all the other parts of the frame, even far beyond

*Article Animal Heat, in Todd's Cyclop. of Anat. and Physiol 1839, t. ii., p. 660.

what could have been presumed as likely or possible, and that in a number of experiments where one hand was plunged in water cooled down by ice, the other hand, which was not subjected to the action of the cold bath, lost nearly 5° R., (11°.25 F., 6°.25 Cs.,) in temperature.

It will be easily understood how important it would be that practitioners should be able to act on the general temperature of a patient, so as to increase or diminish it, by means of a hand or a foot bath. I regret to say that if the facts observed by Edwards are exact, his conclusion nevertheless is incorrect.

I have performed, both alone, and with the help of Dr. Tholozan, Physician of the Hospital *Val-de-Grace*, at Paris, numerous experiments, with the view of knowing whether Dr. Edwards was right or not. I have found that the chilling of one hand plunged in water at the temperature of freezing point, acted very strongly on the temperature of the other hand. But, at first, there is no regularity at all in the quantity of degrees of temperature lost by the hand which remains out of the water; and secondly, we have found once that this hand did not lose any fraction of its temperature. In one case we have observed that the hand kept in the atmosphere did lose 22° F., [12° Cs.] in seven minutes. The ordinary loss of temperature has been of between 6 to 8° F., [3.33 to 4°.44 Cs.] In one case there has been no loss, and, on the contrary, there has been an increase of temperature of 1°.4 F., [0°.8 Cs.]

If, like Edwards, we consider the loss of temperature of the hand not plunged in water as a sign and as a measure of the diminution of the general temperature of the body, we must conclude, from our experiments, that the chilling of a small part of the body may be unable to diminish the temperature of the body sensibly, and that, in other cases, it may act extraordinarily upon it. But the supposition that the hand kept in the atmosphere was able to give a measure of the modification of the body, is incorrect. By taking the temperature of the mouth during the time that one hand was dipped into very cold water, Dr. Tholozan and myself have ascertained that the temperature of the body does not sensibly change. The greatest diminution of the temperature of the mouth has been nearly 1° F., [0°.6 Cs.,] and this only in one

case. In that experiment in which the hand not plunged in water lost 22° F., [12° Cs.] the temperature of the mouth was not diminished more than the fifth of a Fahr. degree.

As it is quite certain that the temperature of the body is constantly changing, it is easy to understand why we do not find a small but a constant diminution in the temperature of the mouth when one hand is subjected to a notable chilling. When, under the unknown influences that are constantly modifying the temperature of the body, there is a tendency to increase that temperature, then the tendency to its diminution originating from the chilling of one hand may exist without producing any effect. We have there two causes acting in opposite directions, and if they are equal they annihilate each other. If they are unequal, we perceive only the difference between them. When these two causes act in the same direction, then their efforts are added to one another, and it is probably in a circumstance of that kind that I have once found a diminution of 1° F., [0.°56 C.,] taking place in the mouth.

We have now to examine how the diminution of the temperature of a hand is produced when the other hand is dipped into cold water. *A priori* it is evident that the chilling of the hand kept in the air exists in consequence, either of the arrival of a cooler blood, or in the diminution in the quantity of blood. That hand being exposed to a cold air (for it is only in such a case that the experiments succeed,) loses its temperature by the action of that cold air. At first the blood that arrives in the hand is not cooler, as Edwards had supposed it was. This we prove by the fact that the temperature is but very little changed in the mouth. The supposition then remains that the quantity of blood arriving in the hand is smaller than usual. This may happen by two modes, one of which is that the heart sends less blood, and the other that the blood-vessels of the hand are contracted and prevent, in part, the passage of blood. It is certain that the heart continues perceptibly to send the same quantity of blood. Therefore we are induced to admit that the hand's blood-vessels are contracted. But now what is the cause of that contraction? We will try to show that it is in an action of the nervous system. Every one knows

that under the influence of a sensation or of emotion, the hands, and sometimes the feet, become cold. The nervous system, in consequence of that sensation or emotion, acts upon the blood-vessels and excites them to contract. The calibre of the visible vessels is sensibly diminished. The same phenomena take place in the two hands when one is dipped into very cold water. An exceedingly violent pain is felt, the nervous centres are strongly excited, and they act then as under the influence of an emotion. Dr. Tholozan and myself have observed that the greater the pain felt, the more the temperature was diminished in the hand left in the air.

As to the influence of partial heating Dr. Edwards relates the following experiment:

"The hand being immersed in water heated to the temperature of 34° R., [108°.5 F.—42°.5 Cs.,] rose one degree of the same scale, and the temperature of other remote parts not immediately exposed to the influence of heat were found to have risen to a corresponding degree."

I have repeated this experiment of Dr. Edwards, and I have found no evident elevation in the temperature of remote parts, as the mouth and the hand, not immersed in water.

I conclude then, from the facts contained in this note, that, in general, the temperature of the body is not sensibly modified by the chilling or the heating of a small part of the frame.—*Ib.*

Case of Conception before the Appearance of the Menses. By Wm. T. Taylor, M. D., Philadelphia.—The general experience of the medical world has established it as a physiological fact, that conception cannot take place prior to the appearance of the menses. But there are instances of females becoming mothers who have never menstruated; they, however, must have conceived just at the time when the catemenia were about to be established, which after parturition and suckling probably occurred regularly.

The following case, which I have met with, seems to be an anomaly in the annals of midwifery:

During the month of June, 1851, I was requested to

visit Hannah B , a mulatto, who was pregnant with an illegitimate child. Was much surprised at finding my patient herself was a mere child in appearance and manner. She was thirteen years of age on the 3d of the previous February, and, though she had never menstruated, was, when I saw her, in the eighth month of gestation. Her general condition was plethoric; her breasts well developed, and the areola quite dark.

On the 13th of August she was taken in labor, but in consequence of a prolapse of the funis umbilicalis, which could not be replaced, I delivered her of a still born child, of the usual size, and perfectly formed. The lochia continued for a few days, and she passed through the accustomed period after delivery very favorably.

It is now one year since, and her menses have not yet made their appearance, nor has there been any vicarious discharge; her health during the whole time remaining perfect.

Never having read of a case of the kind, I have sent it to you for publication, should you think it of sufficient interest.—*Ib.*

Extraordinary Case of Hydrocephalus.—A post-mortem examination was made, (by order of the coroner in London, on the 5th of Sept. 1852,) of the body of Joseph Devine, aged fourteen years, found in the Regents Canal, having fallen into the water while being attacked with a fit. The head measured twenty-seven and a quarter inches in circumference; seventeen and a half inches across the top from ear to ear; nineteen and a half inches from the nape of the neck up to the centre of the back, over the crown to the junction of the eyebrows; eleven and three-quarter inches from one parietal bone to the other. The skull was as thin as that of a child two years old, and the sutures were open like those of an infant, never having closed. When punctured, upwards of five imperial pints of water escaped from it, and the substance of the brain itself weighed three and three-quarter pounds. With the exception of the celebrated case of Cardinal, who lived to the age of thirty-two, is the largest head on record.—*Ib.*

THE WESTERN JOURNAL.

THIRD SERIES.—VOL. X.—NO. V.

LOUISVILLE, NOVEMBER 1, 1852.

THE KENTUCKY STATE MEDICAL SOCIETY.

The readers of this *Journal* will remember that we gave an account, last year, of the formation of a State Medical Society, in Kentucky. Last year the Society was formed, and, after organizing, and preparing work for a future meeting, it adjourned to meet in Louisville on the 20th October, 1852. The recent meeting, therefore, may be regarded as the first meeting of the Society, and we are sure that we express the feelings of all the members when we say that it is scarcely possible that a more pleasant meeting of the kind ever was held. Every one seemed to be animated by one feeling, and that was devoted to the success of the cause. The physicians of Louisville united themselves in numbers with the Society, and every thing was conducted with the greatest harmony and good will. We do not remember ever to have seen a larger amount of business completed by any Society in the same space of time.

When the Declaration of Independence was announced, it made no change in the actual state of the contest with Great Britain. It announced a position; it proclaimed a resolution, a determination to

accomplish a result; but how far that result lay in the future, it did not proclaim nor indicate. But it accomplished some great ends. It made those who were engaged in the lingering and disputed struggle feel that they had a country; that her head was erect among the nations, and that all who were engaged in the cause were many degrees above the character of mere rebels. In a miniature form these must be the feelings of those who witnessed the sessions of the recent meeting of the State Medical Society in this city. The physicians of Kentucky must rejoice to know that they have a rallying point now for professional advancement, and professional honor. All must now feel that the medical men of Kentucky have harnessed themselves for their duties to humanity, to science, and to their profession. These were the feelings that animated us as we looked upon the proceedings of the Society, and they were strengthened by all that we saw. It was an animating scene to look upon those who had attained the highest positions in their profession, as well as those who occupied humbler places, working sturdily together towards a common point, and we felt that the glory, honor and dignity of a profession, which, in the expressive language of the Louisville Chancellor, "is, at least, next to the profession of the legate of the skies," were in hands that would protect them. May the spirit of labor, and devotion to the interests of medical science, that moved the members of the Society at its recent meeting, ever characterize the movements of the association, and thus advance alike the interests of the profession and of society.

As a matter of interest to the profession, we publish a synopsis of the proceedings of the Kentucky State Medical Society, at its recent meeting in this city.

At the second annual meeting of the State Medical Society, held in the Circuit Court Room, in Louisville, on the 20th day of October, 1852, at 1 o'clock, P. M., the President, Dr. Wm. L. Sutton, took the chair, and called the Society to order.

Dr. J. B. Flint offered an amendment to the second section of article three of the Constitution. The amendment is for the purpose of increasing the facilities for admitting members. It was not acted upon at this session, and lies over for consideration next year.

The roll of members was called, and a considerable proportion o the original members answered to their names.

A large number of new names was offered for membership, and, during the progress of the session, some seventy new members were enrolled.

AFTERNOON SESSION.

The Committee appointed to report on the order of proceedings, reported a series of recommendations. which was adopted. One of the series, which provided for the nomination of the officers of the Society by a committee, was afterwards stricken out.

On motion of Dr. Gross, the thanks of the Society were given to the Indiana State Medical Society for copies of the proceedings of the third annual meeting of that Society, and, upon motion of Dr. L. P. Yandell, Drs. Clapp, Leonard and Steele, of New Albany, Ia., who were present, were invited to take seats with the Society, and participate in the proceedings.

Dr. Bell presented the form of case-book modelled by the President of the Society, which he moved to refer to a committee to report upon. Committee: Drs. Bell, Foree, and Chipley.

On motion of Dr. Raphael, a committee was appointed to draft a set of by-laws for the Society. Drs. Raphael, Darby, and Drane, committee.

On motion of Dr. Bell, the order of proceedings was so far modified as to permit Dr. Chipley, Chairman of the Committee on Vital Statistics, to make his report. The report is one of great interest, the investigations were thorough, and the writer accompanied the report with a map, by which the relative sanitary conditions of the various counties were designated.

On motion of Dr. Evans, the report was received and referred to the committee on publication.

The Society adjourned to meet at 7½ o'clock, P. M., to hear the address of the President.

According to adjournment, the Society met, and devoted a short time to the business of admitting members, and the nomination of members for honorary membership. Dr. Chipley nominated Prof. E. Bartlett, of New York. Dr. Gross nominated Prof. Drake, of Ohio. And Dr. Yandell nominated Dr. Deaderick, of Tennessee, as honorary members, which were received for future action.

Prof. Miller offered the following resolution, to lie over until next day, for consideration:

Resolved, That the members will faithfully comply with the requisitions of the registration law passed at the last session of the Kentucky Legislature, and do what they can to have it complied with throughout the State.

Dr. Bell moved that Professor Caldwell be invited to meet the Society during its sittings, and participate in its deliberations. Passed unanimously.

The President, Dr. Sutton, then read the annual address, replete with matter of great interest, and with sound advice for elevating the character of the profession. On motion the address was referred to the committee on publication.

The Society adjourned until ten o'clock the next day.

SECOND DAY'S PROCEEDINGS.

Oct. 21—10 o'clock. The Society was called to order by the President. After the reading of the minutes, Dr. Gross moved the formation of the following special committees:

1. On Medical Biography, or the lives of meritorious or distinguished physicians or surgeons of Kentucky.
2. On Medical Literature, or the history of the medical authorship of Kentucky.
3. On the relation between diseases and peculiar geological formations.
4. On the Statistics of Hernia.
5. On the Statistics of Lithotomy and calculous diseases.
6. On the History and mode of management of Hospitals, Asylums, Infirmaries, Penitentiaries, and Prisons.

[This committee was afterwards divided, by assigning the two last named institutions to one committee, the others to another.]

7. On suits for malpractice.
8. On the results of surgical operations in malignant diseases.
9. On Epidemic Erysipelas.
10. On Epidemic Dysentery.
11. On Typhoid Fever.
12. On Placenta Previa.

And, at the suggestion of Dr. Bell, a Committee on the Statistics of Remedies.

Invitations were received from the Louisville University, the Kentucky School of Medicine, the Louisville Marine Hospital, and the Institution for the Blind, requesting the Society to visit those Institutions. All of which were received.

Professor Gross moved that Dr. Drake, of Cincinnati, be invited to take part in the deliberations of the Society.

Dr. D. D. Thompson, on behalf of the physicians of Louisville, invited the Society to a supper at the Louisville Hotel, on Friday night.

Prof. Flint moved that the election of officers should now be made, which was carried. After various ballotings, Dr. Chipley, of Lexington, was declared the choice of the Society for President. Drs. Bell and Drane were appointed a committee to conduct the President to the Chair.

Dr. Peter moved that a committee for nominating the remaining officers be appointed, consisting of one from each county, and one from the city of Louisville. The motion was carried, and the following gentlemen were appointed.

Dr. Darby, of Fayette; Dr. Bell, of Louisville; Dr. Owen, of Henry; Dr. Spillman, of Mercer; Dr. Sutton, of Scott; Dr. Chew of Woodford; Dr. Freeman, of Oldham; Dr. Foree, of Jefferson; Dr. Sneed, of Franklin; Dr. Letcher, of Jessamine.

Dr. Gross moved that the standing committee report in the order in which they stand in the proceedings of last year. Adopted; and on call, several of the committees failed to report. Dr. Evans, of Covington, read an able and well written report on Medical Ethics, which was received and referred to the committee on publication.

Adjourned to three o'clock, P. M.

AFTERNOON SESSION.

Dr. Chipley in the Chair. The society met pursuant to adjournment, and after the election of several members, the nominating committee reported candidates for the various offices; and upon a ballot for each office, the following officers were declared elected:

Dr. E. C. Drane, of Newcastle, Senior Vice President; Dr. J. W. Knight, of Louisville, Junior Vice President; Dr. Sneed, of Frankfort, Recording Secretary; Dr. R. J. Breckenridge, of Louisville, Corresponding Secretary; Dr. D. D. Thompson, of Louisville, Treasurer; Dr. B. Monroe, of Frankfort, Librarian; Drs. Bell, Foree, and Ronald, Committee of Publication.

Dr. Miller, chairman of the Committee on Obstetrics, then read a Report on Chloroform in Midwifery, and the use of the speculum in uterine diseases. The report commanded the profoundest attention; and that portion of it which examined the sentiments of Dr. Lee, of London, contained a triumphant refutation of that gentleman's opinions. The report was received and referred to the committee on publication.

Dr. Sutton read a report on Registration, which was read and referred to the committee on publication.

Dr. Jacobs offered a resolution respecting the clause in the constitution which restricts honorary membership to eminent physicians residing out of Kentucky. Ordered to lie over until the next day.

Adjourned to 7½ o'clock, P. M.

EVENING SESSION.

Dr. Chipley in the Chair. After the election of members, Dr. Wible offered the following preamble and resolution:

WHEREAS, Physicians are frequently called upon to give evidence in courts of justice, to make post mortem examinations, and institute investigations in cases of poisoning, services of a strictly professional character, requiring expense, time, and labor on the part of the physician;

AND WHEREAS, these services are often of great importance to the welfare of society; and as physicians ought not to be expected to perform them without remuneration; it is the opinion of the Kentucky State Medical Society, that the interests of humanity demand that laws be enacted which will properly secure these services when required by coroners and other officers of the law;

Therefore, be it Resolved, That a committee of three be appointed to prepare a report, to be read at the next annual meeting of this Society, on the subject presented in the foregoing preamble.

[We may as well state that Dr. Sutton introduced a resolution for the appointment of a committee "to consider whether any, and if any, what means can be brought into requisition to lessen the heavy burthen of pauper practice, and report at the next annual meeting." The resolution and Dr. Wible's were referred to Dr. Wible, as chairman of the committee, with power to appoint coadjutors.]

Dr. Miller's resolutions, of yesterday, were adopted.

Dr. Darby, as chairman of the committee on Epidemics, read a report for himself, and one from Dr. B. P. Drake; and Dr. Allen, of the same committee, also read a report on Epidemic Dysentery.

The Society then selected Lexington as the place for the next annual meeting.

Adjourned to meet at 10 o'clock to-morrow.

THIRD DAY'S PROCEEDINGS—MORNING SESSION.

Dr. Chipley presiding, Dr. Gross's resolution for special committees was called up and adopted. Dr. Darby reported By-Laws, which report was suspended in order to hear Dr. Gross's report on Sur

gery. This admirable report, one of the finest contributions that has been made to the medical literature of Kentucky, excited the deepest interest; and although expectations were very high, we believe the Professor transcended all anticipations. The report contains the most complete history of Kentucky surgery that has been written, and it would be considered a first class paper among the éloges of the French Institute The report was received and referred to the committee on publication.

Dr. Spillman made a remarkably excellent report on Indigenous Botany, which was referred to the committee on publication.

The following resolutions, appended to the report, were unanimously adopted:

Rvsolved, That this Society regard the cultivation of our own Botany as essential to a full development of our own professional resources, and to a more successful practice of our art.

Resolved, That the profession throughout the State, and the members of this Society particularly, be requested to give special attention to this subject, and by cultivating the field of observation and research, in connexion with the unexplored regions of vegetable nature within our own limits, ascertain to what extent the demands of the healing art can be supplied at home.

Resolved, That any physician discovering a new remedy, or a new property in one already known, or any information touching the Medical Botany of our State, that can be rendered practically available, be requested to communicate such information to the chairman of the committee on Indigenous Botany.

Resolved, That the committee of Publication, at as early a day as practicable, by a brief circular or otherwise, communicate the objects embraced in these resolutions to the profession throughout the State, giving the name and location of the chairman of the committee on Indigenous Botany, and earnestly requesting contributions.

Dr. Raphael's report on By-Laws was presented, received and adopted. Our limited space forbids the publication of these by-laws in the present number of the Journal.

Dr. Anderson offered a resolution of thanks to Col. Buckner for the use of the Court Room, and attentions to the Society. Unanimously adopted.

Various nominations for membership were acted upon, and the ballot was favorable in all instances.

The following appointments were announced by the President:

STANDING COMMITTEES.

Committee of	Arrangements.....	Dr. Darby, of Lexington.
"	on Practical Medicine..	Prof. Foree of Louisville.
"	on Improvements in Pharmacy.....	Prof. Silliman, "
"	on Vital Statistics.....	Dr. Sutton, of Georgetown.
"	on Obstetrics.....	Prof. Powell, of Louisville.
"	on Medical Ethics.....	Dr. Hewett, "
"	on Public Hygiene.....	Dr. Bell, "
"	on Epidemics.....	Prof. Bullitt, "
"	on Surgery.....	Prof. Flint, "
"	on Indigenous Botany...	Dr. Emmert, of Pike county.
"	on Finance.....	Dr. Letcher, of Jessamine.

SPECIAL COMMITTEES.

1. On Medical Biography.....	Dr. R. J. Breckenridge, of Louisville.
2. On Medical Literature, &c.....	Prof. L. P. Yandell, of Louisville.
3. On Relations between Diseases and particular Geological Formations	Prof. Peter, of Lexington.
4. On the Statistics of Hernia.....	Dr. S. B. Richardson, of Louisville.
5. On the Statistics of Lithotomy and Calculous diseases.....	Prof. Gross, of Louisville.
6. On the History and Management of Hospitals, &c.....	Dr. Raphael, "
7. On Penitentiaries and Prisons....	Dr. Sneed, of Frankfort.
8. The results of Surgical Practice in malignant diseases.....	Dr. Colescott, of Louisville.
9. On Epidemic Erysipelas.....	Dr. Owen, of Newcastle.
10. On Epidemic Dysentery.....	Dr. Hynes, of Bardstown.
11. On Typhoid Fever.....	Dr. C. H. Fry, of Louisville.
12. Placenta Previa	Prof. Miller, "
13. On the Statistics of remedies in disease.....	Prof. Rogers "

Dr. Thompson, Chairman of Committee on Finance, made a report rom that committee. The report did not include the receipts at the re-

cent meeting. Dr. Bell, Chairman of Committee on Case-Book, reported. He stated the extraordinary labors of Sir W. F. Chambers, of London. That gentleman has been engaged for many years in a most laborious private practice, in the management of a large hospital, and in teaching. He was also very sickly, yet he has kept a minute record of all the phenomena of all his cases, and has filled sixty-seven quarto volumes, each of 400 pages, with records of his cases. These records are all written in Latin. Sir William also made a number of Index books to his quarto volumes, and for years wrote up the hospital books in the style of his private case-books.

The committee urged the absolute necessity of a case-book for every practitioner, in which he may record the symptoms of every case. Two resolutions were appended to the report. The report was referred to the committee on publication, and the resolutions unanimously adopted. The following are the resolutions:

Resolved, That the Kentucky State Medical Society commend to the physicians of the State, and urge upon the members of the Society, the purchase of case-books of the kind described in this report.

Resolved, That in order that the publisher may determine how far he may be justified in putting this case-book to press, that the members present who may wish to encourage this publication, hand their names to the committee on publication, and that the committee shall be authorized to continue to receive these names after the adjournment to the next annual meeting, until a number shall be obtained sufficient to justify the enterprize; and when the committee shall have received this assurance, they shall be authorised to make arrangements with the publisher: Provided, the Society shall not be committed in any way for the expense of publication.

Dr. Raphael offered the following resolutions:

Resolved, That if the amount in the Treasury should not be sufficient to defray the expenses of getting out the proceedings of the Society, that the publishing committee assess the members *pro rata* with such excess.

Resolved, That any member may obtain one copy or additional copies, extra, of the transactions, by paying the cost price of the same.

Resolved, That the President be authorized to fill any vacancies in the delegation to the American Medical Association.

The following delegates were elected: Dr. Chipley, of Lexington, Dr. Freeman of Oldham; Dr. W. C. Sneed, of Frankfort; Dr. Raphael, of Lonisville; Dr. T. S. Bell, of Louisville; Dr. Gore, of Bloomfield; Dr. R. J. Breckenridge, of Louisville, Dr. Moore, of Harrodsburg; Drs. Wible, Hardin, and Hewett, of Louisville.

Prof. Bartlert, of New York; Prof. Drake, of Ohio; and Dr. Deadrick, of Tennessee, were then unanimously elected honorary members.

On motion of Dr. Jacobs, the thanks of the Society were tendered to the officers for the faithful, able, and impartial manner in which they had discharged their respective duties.

The Society then adjourned to meet at Lexington on the third Tuesday in October, 1853.

Thus closed the labors of this Society, and we sincerely believe that the meeting not only did a great service for the Medical profession, but that the good results will continue to show themselve for years to come.

We understand that the volume of Transactions will be put to press immediately; and it will be a book of which the medical men of Kentucky will have reason to be proud. We earnestly hope that every medical man in the State, who feels any interest in his profession, will order a copy of the work. The principal Reports will form a portion of the volume.

We understand, too, that the Committee of Publication are making arrangements with Mr. T. Nelson, of this city, for the immediate publication of the case-book referred to in the proceedings. It will be a proud day for medical science and practice in Kentucky, when all the practitioners of the State commence the use of the case-book. Those who begin it, will find such benefits to themselves and patients, that the practice will not be abandoned speedily. B.

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THE

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OF

MEDICINE AND SURGERY.

DECEMBER, 1852.

Art. I. *Laryngismus Stridulus.* By J. W. Bright, M. D., of Louisville, Ky.

This is a disease of the Thymus gland of infants, and takes its name from the stridulous, or crowing, croaking sound in breathing. It assimulates croup and bronchitis in many of its symptoms. And the true character of the disease is detected in its early stage only by applying the ear to the chest of the patient. Delicate or scrofulous children, from the age of three or four months, to three or four years, are most liable to this disease. Males are more subject to it than females. It makes its onset by slight paroxysms of fever, not preceded by any perceptible chill or cold stage. The child appears to have taken cold. The cheeks are flushed, and the pulse is more or less accelerated. The breathing is more hurried than

usual, and difficult, owing to the pressure of the enlarged thymus gland upon the trachea. It is attended by a short troublesome cough. The phlegm is loose, but raised with difficulty, and when discharged, is white and frothy. The child is disposed to throw its head back, with its mouth open, and roll its tongue from side to side, with a chewing motion of the jaws. If it is old enough to raise its hand, it will make frequent efforts to thrust its fingers into its mouth. The phlegm appears to be loose and rattles, but no discharge is made by the effort of coughing. The bowels are inclined to be constipated; but when evacuations are obtained, they are not unnatural, except phlegm has been swallowed. The skin is sometimes dry and hot, but generally it is relaxed, and a cool sweat stands on the forehead; after a day or two has elapsed, the superior portion of the sternum is raised, and there is a fulness above the clavicles. The child sleeps but little, owing to the difficulty of breathing. The countenance is anxious, the complexion varies from a glossy paleness to a livid pallor. There is great thirst, and the child will take with avidity anything that is presented to its mouth. The pathology of this disease is often mistaken, and it is treated as for croup or bronchitis: and the patient falls a victim to a misdirected treatment.

Treatment.—This, as all other diseases, should be treated as the pathology requires.

First, relieve the stomach and bowels by a mercurial cathartic.

Secondly, keep down fever by the Spirits of Mindereri, and Spirits of Nitre, in the proportion of seven of the former to three of the latter. This should be given in repeated doses every hour, of ten drops, more or less, according to the age and strength of the patient. At the same time an ointment of the Iodide of Potassa (officinal) should be frequently applied over the sternum and above the clavicles. When the patient is free from fever, a solu-

tion of the Hydriodate of Potass, ten grains to the ounce of distilled water, should be given in ten drop doses (more or less, according to the age of the patient,) every two hours. The bowels should be kept open with small and frequently repeated doses of calomel, Ipecac, and Nitre. If the phlegm accumulates, and is not freely discharged, an emetic of alum and honey should be given. This is prepared by mixing one drachm of pulverised alum in one ounce of strained honey. Give a teaspoonful every ten minutes till the child pukes freely, and discharge the phlegm. This emetic may be repeated as often as the circumstances require, as it does not debilitate the child, nor leave nausea after vomiting. If these remedies be persevered in for a few days, the disease will yield and the patient recover. If the skin be hot and dry at any time, the tepid bath may be used with advantage. The child should lie with its shoulders and head a little elevated.—In this position it breathes easier. Before the child is weaned, the breast milk is the only diet necessary for it. After weaning, the diet should be light and thin. No recovery can be effected until the thymus gland is reduced. Great care should be taken after recovery that the child be not exposed to cold or dampness, otherwise it will relapse. At the same time, pure fresh air without exposure, will confirm the cure.

LOUISVILLE, KY., December, 1852.

ART. II.—*A Case of Stone in the Bladder, at Astoria, Oregon.* By ISRAEL MOSES, M. D., Surgeon, U. S. A.

George Washington, a colored man, aged about thirty-three years, born in Virginia, and brought up in Missouri, applied to me for medical advice, February 15, 1851, at Astoria, Oregon Territory, and gave the following account of himself: From his earliest recollections he has been

troubled with symptoms of urinary disease, and has been told by his friends that they began to show themselves at two years of age. He suffered at intervals with acute stinging pain, running from the neck of the bladder to the head of the penis, his water being, at these times, of a greenish yellow tinge, and feeling, as it passed from him, as if mixed with sand; but he never could see any, and had no opportunity of examining satisfactorily, from the fact of making water out of doors. During the paroxysms "a sudden motion of the leg would relieve him by shoving something back." During the intervals he enjoyed good health, and was able to work as well as anybody. At the age of twenty-one he was attacked with severe pain about the neck of the bladder with a sensation of something rough, running backward and forward along the urethra, which continued ten days, and was relieved by parsley and water-melon seed tea. From this time he continued well, and was able to work as a field-hand, and thought himself entirely well until about two years ago, "when he had a bad spell," which lasted sixteen or eighteen days.

He started for Oregon on the 6th of May, 1850, walked and drove a team nearly all the way, (upwards of two thousand miles!) arrived on the 30th of August, and continued well a month, when the former symptoms returned; but he worked until the middle of October, when he was obliged to give up. He was also attacked with diarrhea, which continued until the latter part of January. During these *paroxysms*, after passing a little urine, the stream would suddenly stop, and he felt a desire to strain, "but this only made the matter worse, and he was obliged to ease off," so as to evacuate the bladder by drops. He has never been examined by any surgeon, but various physicians have told him, "it could not be stone on account of its coming on him at so early an age." He is now in good general health, complaining only of a ten-

dency to diarrhea. Upon examination with a steel sound, I soon confirmed the diagnosis formed from the rational symptoms: the instrument struck a stone as soon as it reached the bladder. I placed him in my hospital upon a preparatory course for lithotomy.

March 10. Has been very quiet since his entrance (on the first,) with little or no pain, until yesterday, when he had a paroxysm of suffering. His diet has been rice, tea, toast, &c; his bowels kept regular; has slept well, but been obliged to get up two or three times during the night to pass his water. To-day he has a great deal of pain at the neck of the bladder and end of the penis. Directed flaxseed tea, and two grains of opium three times a day.

16th. The patient's general health is excellent; but he has suffered for two or three days from increased pain, and I find him this morning walking up and down the ward with his penis in hands, in great agony. Having obtained a case of instruments only the day before, I appointed 1 o'clock, P. M., for the operation, and directed an enema to be administered, which operated freely before the time.

With the assistance of Dr. Tranchard, Lieut. Talbot, U. S. A., and George Gibbs, Esq, I proceeded to operate. Having placed the man in the usual position, and introduced the grooved staff, which was accomplished with difficulty from the restlessness of the patient, and his endeavors to free himself from his bonds. I had provided chloroform, and began to administer it as I introduced the sound, but it was some minutes before he was fully under its influence. Having struck the stone, so as to convince every one of its presence, I made the external (bilateral) incision, at which he wriggled a little, but soon became perfectly quiet, and I cut down to the staff and into the bladder, using a probe-pointed straight bistoury. Introducing my fore-finger and the

forceps, grasped the stone, which proved to be much larger than I had anticipated. After working gently, I increased the extractive efforts, but found it impossible to bring away the stone without a larger incision. I accordingly withdrew the forceps, and introducing the bistoury along my finger, enlarged the opening in the bladder. Still I found it impossible to extract it. Having seized the stone, however, in a firm grip, and brought it as near the opening as I could, I cautiously nicked away, until I succeeded in its extraction. During the operation, which lasted about ten minutes, the patient remained insensible, occasionally making an effort to free himself, but again becoming perfectly quiet. Dr. Tranchard continued to administer chloroform, and to watch his pulse during the operation. The other gentlemen afforded me their useful assistance.

About ℥xx blood were lost from the division of a small artery. His pulse continued full and regular. As soon as the calculus was brought away, the chloroform was discontinued, his face dashed with cold water and aqua amonia held to his nose—he soon revived, began to sob and talk wildly, but soon became quite sensible. A female catheter was introduced into the bladder, through the wound, and twenty drops Magendie's Sol. Morphine administered. I visited him an hour after, and found he had lost ℥ viii or x of blood; he was restless and just seemed conscious that the operation had been performed; he shook hands and thanked me; complained of some pain in his penis. ℞. Repeat the morpine.

At 6 P. M., the bleeding still continued, losing ℥ viii since my last visit; has slept some, and been easy, but begins to be restless; moans and complains of pain in his penis and in his back, from lying in one position. ℞. Sulph. Morphia gr. x. At 7, P. M., free from pain, and has slept considerably; lost about ℥iv of blood, but no oozing now; pulse 80; skin cool and comfortable. 10, P. M.

Has slept most of the time and continues free from pain; some dropping of blood; skin warm and soft; pulse 90, good. Gave 15 drops morphine. A little after midnight, found him dozing and quiet; free from pain; slight oozing of blood, mixed with urine, through the catheter. I directed the Hospital Steward, who sat up with him, to give 10 drops morphine every two hours, so as to keep him fully under its influence.

17th. Found him very comfortable; had passed a good night, and is free from pain; no bleeding; urine flowing constantly through the catheter. I had him washed and dressed, and put in a comfortable bed. He tells me that he was *perfectly unconscious* of what was going on during the operation; he was sensible of the sound being passed into the bladder and striking the stone, but felt no pain afterwards, though 'he seemed to be in a kind of misery,' as he expresses himself, during the time he was under the influence of the anæsthetic.

18th. Slept well during the early part of the night; took 20 drops of morphine, which composed him, though he says he did not sleep much. Complains only of pain in his back and side; tongue clean and moist; skin cool; pulse 72, natural; passed some water through his penis this morning.

19th. Doing well; not a bad symptom, and no complaint of any pain; the wound looks healthy, and is filling up. I removed the catheter.

20th. Complains of pain along his right thigh and leg; general condition good; has some uneasiness in his bowels, not having had any dejection since the operation; skin warm; pulse 100; has passed some urine through the penis, but mostly from the wound. ℞. Castor Oil, with peppermint water.

21st. Complained last evening with pain and uneasiness about the bowels, and some tenderness on pressure. As the oil had not operated I repeated the dose, which ope-

rated three times, and he slept well during the remainder of the night. This morning he is groaning a good deal and complaining of pain in his right hip and leg. Has passed no urine per urethra. I introduced a gum elastic catheter, but it did not enter the bladder, the passage appearing to be plugged up by coagulated blood and lymph, which filled the eye of the catheter; his general condition is excellent; the wound looks healthy, and is almost closed.

22d. Complained yesterday of a severe headache, and about half-past five P. M., was seized with a severe rigor; during which, however, his skin was comfortably warm and perspiring; pulse 100, rather small, followed by hot and dry skin, frequent pulse, coated and sticky tongue. At the onset of the chill, he took 20 drops morphine. During the day he passed his water freely through the penis, in a stream. ℞. Pil Mass. Hydrarg., gr. x, at night.

23d. Is quite comfortable this morning; took castor oil, ℥ss, which operated twice or three times; is free from pain; has passed very little urine per urethra; introduced a catheter and drew off ℥viii, with a good deal of pus mixed with it. Complains of some pain in his right iliac region; no tenderness on pressure; pulse 84, natural; tongue clean and moist; wound healthy in appearance.

26th. Has gone on improving steadily, passed nearly all his urine by the penis, and is generally in most excellent condition. On the last day of the month, I discharged him perfectly well. He had to pass his urine more frequently than one in health, but the bladder was fully under his control.

The stone weighed five hundred and nine grains; an inch and a half in its longest diameter, nearly cubical, with rounded angles; surface rough, with mammillary projections, and studded with fine semi-transparent crysals of oxalate of lime; the projections composed of con-

centric coats of light greenish ox. lime and ox. iron (? the latter appearing in brown highly polished incrustations, which show themselves when the greyish coat is denuded.

ART. III.—*A Case of Hematemesis.* By DR. G. M. BENNET, of Smithsville, Indiana.

A. M., aged 52 years, living in a malarious region, temperate, a laborer, who had previously enjoyed good health, was seized with a chill on the 6th of October, after a few days of indisposition. I saw him in the fever which succeeded, but did not prescribe for him, as it was his wish to treat his own case. He took some purgative pills of a drastic nature, and a little quinine, eating during the day a piece of pickled cucumber. Soon after the pills began to operate he was attacked with nausea and vomiting, and it was at once perceived that he was throwing up blood from his stomach. Blood also passed from his bowels.

Oct. 7th. About 3 o'clock, P. M., I saw him, when he was in the following condition: He was vomiting blood at intervals of fifteen minutes, and passing it from his bowels; great thirst; tenderness in the epigastrium; extremities cold; breathing laborious or gasping; pulse scarely perceptible at the wrist; extreme restlessness. I prescribed cold drinks instead of the hot stimulants that had been given; cupped freely over the stomach, and followed the cupping with fomentations; and used sinapisms to the extremities. Still the paroxysms of nausea and vomiting continued to recur at short intervals, during which large quantities of blood were evacuated upwards and downwards. The blood discharged in vomiting was of the bright arterial hue; that from the bowels was less florid. The vomiting afforded only partial relief to the nausea, and the stomach continued to be oppressed.

Under these circumstances, I determined to try the effects of an emetic, and accordingly administered a full dose of ipecacuanha in a glass of cold water.

Under the operation of the emetic his pulse rose and the symptoms all improved. I gave him then two grains of the acetate of lead, and half a grain of sulphate of morphia.

In a short time the symptoms were as alarming as ever, and I resolved to repeat the emetic, encouraged by its good effects in the first instance. The effect was decided. His pulse rose, and the patient felt so much relieved that he began to speak of recovering. The lead and morphia were again given, and after a few hours the emetic was given the third time.

No hematemesis occurred after the last emetic, and his circulation became good. I gave him then a dose of calomel combined with morphia, which acted well on his bowels, bringing away large quantities of feces stained with blood.

8th. Patient remained comfortable till evening, when the nausea and sense of sinking returned; his extremities again grew cold, and he complained of tenderness in the abdomen, in the region of the descending colon; cups and fomentations were applied over the seat of abdominal tenderness, sinapisms and frictions to his extremities. The stomach again grew excessively irritable, and everything taken into the stomach was quickly rejected. Opiates, stimulants, and astringents were administered with only very slight advantage.

9th. The same state of things as that which prevailed yesterday still continues. The paroxysms of vomiting return hourly, or oftener; his thirst is very great, and his bowels are moved as often as he vomits; extreme restlessness, with suffocative breathing; the matter from his bowels has the appearance of dirty water, almost destitute of feculent matter. As the only thing that seemed

to benefit the patient at all was the emetic, I determined to have recourse to it once more, and accordingly administered a dose of ipecac. in a little warm water.

The first influence of the emetic was alarming—he seemed to sink under its action; but reaction soon came on, and for some hours the patient was more comfortable. At the end of this time the spontaneous vomiting returned, with the serous purging; his extremities grew cold as high as the elbows and knees, and he was pulseless at the wrists. Wine and opium were freely given, and friction of the extremities was vigorously employed. Applied a blister to the epigastrium.

10th. Patient a little better. Blister has drawn well. The symptoms of yesterday persist, but in a mitigated degree. His stomach will retain nothing but the wine and opium, which are freely given. The opium does not narcotize him. Appearance of the patient much that of a man in the collapse of cholera.

Evening. A change is observable in the character of the alvine dejections, which are slightly feculent, with a tinge of bile, as if the calomel which I gave the day before, and supposed he had thrown up, might be producing some effect. Wine and opium to be continued.

11th. Patient better; stomach more tranquil, discharges from bowels still not satisfactory. Ordered a full dose of calomel and morphia.

The operation of the calomel was good, and the discharges favorable. His kidneys also began to act; his circulation was improved and nearly natural, except when affected by nausea. Stomach will retain no nourishment; little appetite; much thirst. Continue the stimulants and the opiate.

12th. Bowels have been quiet for some hours, and the patient is more comfortable. To-day he has some fever. This fever continued for a few days, being of a typhoid type; but it was not obstinate, and the patient went on improving, and is now well.

I am not sure that I understand the character of this case, but suppose that the affection was of malarious origin. The subject had suffered with chills, and his disease was probably a masked intermittent—one of the *febres intermittentes sub forma larvata*, of the old writers. A congestion of the portal circle induced by the malarial poison might naturally find vent in hemorrhage from the stomach.

I am the more inclined to this view of the subject from the fact that I have heard of the occurrence of a case similar to the foregoing in the practice of a medical friend. This patient was also a resident of a malarious district, and had been affected with the symptoms of an obscure intermittent. The quantity of blood discharged by emesis was very great—a chamber-pot was nearly filled, and the bed-clothing was saturated with blood. The hemorrhage recurred three or four times, at intervals of two, four, and six days, prostrating the patient to the last degree. He however recovered.

It is true that these cases, if cases of "intermittent fever," were very obscure, but they seem to me to bear more of that character than any other.

OCTOBER 25, 1852.

SELECTIONS

FROM

FOREIGN AND HOME MEDICAL JOURNALS.

On Variations of Pitch in Percussion and Respiratory Sounds, and their application to Physical Diagnosis. By AUSTIN FLINT, M. D., of Buffalo, New York.—Very litttle attention has hitherto been paid to variations in the pitch of sounds heard in the practice of percussion and pulmonary auscultation. The sibilant and sonorous râles of bronchitis, it is true, are distinguished from each other chiefly by a contrast in pitch, but, as respects the remainder of the physical signs pertaining to pulmonary disease, they appear not to have been much studied in this aspect, and even the facts that have arrested notice do not seem to have been applied, save in a very limited degree, to physical diagnosis. By most writers on physical exploration, pitch modifications, except* in the sibilant and sonorous râles, are not recognized, no allusion whatever being made to them. In the second edition of the able and comprehensive work of Dr. Walshe, of London, recently republished in this country, the subject is noticed more distinctly than by any other author with whose writings I am acquainted. Dr. Walshe enumerates among the elements involved in the different modifications

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* The vocal sign *aegophony* should perhaps also be excepted.

of respiratory sounds in health and disease, variations in pitch; he also mentions several important facts with respect to these variations. But he apparently loses sight of their practical applications, making no reference to them in connection with the diagnosis of individual thoracic diseases. Barth and Roger state, as briefly as possible, the fact that the bronchial respiration is higher in pitch than the cavernous. But, in general, as just remarked, nothing is to be found relating to this subject in standard treatises* on percussion and auscultation.

What facts are disclosed by the study of percussion and respiratory sounds, in health and disease, with reference to variations and pitch? How far are these facts available in diagnosis? The latter question is much the more important of the two; or, rather, the importance of the first question depends mainly upon that which belongs to the second. The subject of physical exploration has already suffered from over refinement. To be practically available in the hands of all intelligent practitioners, the art must be simplified as much as possible. New distinctions, unless obviously tending to enlarge or facilitate our means of diagnosis by physical signs, were they not only true, but ever so interesting to those whose attention is specially directed to this department of medicine, would be of questionable utility, so far as they serve to render the pursuit mere complicated and difficult. In proposing, therefore, to submit the results of my observations and experience, thus far, in answer to the foregoing inquiries, I am influenced by the belief that, considered in this point of view, the physical signs of pulmonary affections admit of being enlarged in their application to diagnosis, and rendered more readily available for practical purposes. A single additional remark by way of introduction; the

* In connection with this statement, it is proper to give the bibliography to which it applies. The works consulted are as follows:—

Laennec, edited by Forbes; Walshe on the Heart and Lungs; Hughes's Physical Diagnosis of the Lungs and Heart; Barth and Roger's Practical Treatise on Auscultation; Gerhard on Disease of the Chest; Prize Dissertations on Physical Exploration, by Drs. Holmes, Bell, and Haxall; Blackiston on Diseases of the Chest; Latham on Auscultation and Semeiology; Swett on Diseases of the Chest, edition of 1852; Bowditch's Young Stethoscopist; Lawson's Lectures, in the Western Lancet.

pitch modifications of sound, as before intimated, opening a field of study in physical exploration as yet but little cultivated, and to which, so far as relates especially to auscultation, my attention has but recently been directed, propriety and prudence dictate, not only caution in making deductions from a number of data somewhat limited, but a certain amount of distrust in a kind of observation in which the liability to error cannot be at once fully estimated. In view of these considerations, the conclusions which I shall present are advanced as propositions to be confirmed by further researches, the object of this paper being, in a great measure, to invite the investigations of others in the same direction.

The subject naturally resolves itself into the study of—First, *percnssion* sounds; and, second, the pulmonary sounds disclosed by *auscultation*.

SECTION 1. *On attention to Pitch of Sound in the practice of Percussion.*—It is hardly necessary to gather recorded data with reference to the variations of pitch of *percussion* sounds, since observations can be so easily made, and repeated, to any extent, by every one. I shall therefore give my experience under this head briefly, and in a general way, *i. e.* without citing particular cases. The use of the term *pitch*, as applied to percussion sounds, may be questioned, and questionable. Whether it be correctly applicable, in a musical sense, to these sounds, or to those of auscultation, I shall not stop to inquire. It suffices if the idea it conveys be intelligible, and if it denote an appreciable distinction. A percussion sound elicited from the chest, if not entirely flat, ha a certain amount of resonance. This resonance may differ in degree; in other words, the sound is more or less clear or dull. A peculiar quality of tone arises from the fact that the chest contains air in vesicles, and hence the sound may be distinguished as the *vesicular resonance.* Percussion sounds present occasionally, deviations from this vesicular quality; an example the most familiar being the change occasioned by the presence of air between the pleural surfaces, or in a large excavation, constituting what is known as tympanitic resonance. Now, in addition to tnese classes of variations, the resonance on per-

cussion in different parts of the chest, in morbid conditions, present a disparity analogous to, if not identical with that which constitutes the distinction called *high* or *low* in comparing musical tones—in other words, a disparity in pitch. This kind of disparity is recognized by directing attention to, and comparing the sounds elicited from different regions, just as is done with reference to musical notes, in determining whether they are relatively higher or lower in key. The faculty of distinguishing musical discords, in other words a musical ear as it is called, doubtless assists this discrimination, and it is probably true that the ability to detect a slight variation will correspond with the delicacy with which deviations from the harmony of musical tones are appreciated.

What pathological significance belongs to the fact of a variation in the pitch of resonance? The practical importance of the subject depends, of course, on the answer to this question. The question leads to the enunciation of the following law: An elevation of pitch always accompanies diminution of resonance in consequence of pulmonary consolidation. In other words, dullness of resonance is never present without the pitch being raised. This proposition is to be verified by observations which may be readily made. Cases of tuberculous deposit, with marked disparity of resonance at the summit of the chest on one side, will furnish examples, and such cases are sufficiently numerous. But the law may be tested in the healthy subject. The præcordial region presents well-marked dullness compared with the corresponding region on the right side. The percussion sound in the former situation will answer as a type of resonance, modified by an increased proportion of solid material incident to tuberculous or other disease

A practical advantage of attention to pitch, in employing percussion, consists, thus, in its confirming the existence of dullness. It furnishes additional evidence thereof, and adds positiveness to the conclusion, when the diminution of resonance might not be with certainty determinable. To the musical ear, more especially if skilled in discriminating musical tones, a disparity in pitch is more quickly as well as more clearly distinguished. It is far easier to appreciate a contrast in this point of view, than to determine a slight or moderate preponder-

ance of resonance in the percussion sound on one side of the chest. I have frequently illustrated this in teaching physical exploration. A person just essaying to distinguish relative dullness of resonance on percussion, often fails in its recognition, even when it is pretty strongly marked. He is unable to perceive a difference which is sufficiently apparent to the practical percussor. Under these circumstances I have frequently inquired if the learner were able to sing, or play on any musical instrument. If he replied in the affirmative, I requested him to compare the sounds on the two sides of the chest, as if they were musical notes, with reference to pitch, and the disparity then became immediately manifest. If I am misled with respect to the assistance to be derived in this way, by the results of my own experience, the error involves a kind of self deception which it is very easy to impart, for many to whom I have pointed out this method of determining dullness, have assured me that they have found it of great utility in practice. By directing attention to the pitch, an intelligent student, with some musical powers and cultivation, will become an expert in appreciating a slight dullness sooner than he can attain to proficiency in the manipulating tact of percussion.

Another practical advantage, certainly equal to, if not greater than the foregoing, is derived from the fact that a distinct disparity of pitch may be apparent, in some instances, when a disparity in the amount of resonance is inappreciable. The correctness of this statement my observations lead me not to doubt. Others, however, are to be satisfied by the evidence of their own perceptions. This fact is stated by Dr. Bowditch in his work entitled the *Young Stethoscopist*, and I do not recollect meeting with a similar statement by any other writer. He says, "A *difference of note or of pitch* between two corresponding parts is not uncommon, when there is no real flatness in either. It occurs in cases in which the lung is not by any means impervious to air. Sometimes in the early stages of phthisis and in pneumonia in its early or latest stages;" (page 60, second edition.) In another place he remarks: "Any degree of dullness, even the slightest *difference of note or of pitch, if confined to the upper part* of the chest, between the portions equi-

distant from the spine or sternum, augments my suspicion of the existence of tubercles;" (page 88, ibid.) To measure the exact amount of mere resonance, so as to make an accurate comparison of two sounds that differ but slightly in this respect, it must be evident on a little reflection, is not easy. The truth probably is that the difference in pitch is perceived when a slight disparity exists which is ordinarily called dullness. And this leads me here to remark, it is not to be supposed that practical chest explorers have not been accustomed to be guided by pitch modifications of sound. Variations in this respect have been perceived without recognition, if this antithesis be allowable. The disparity has been apparent, but attributed solely to diminished resonance or dullness. This remark will be found to apply equally to respiratory or to percussion sounds. This, however, does not prove that the practice of physical exploration will not be materially aided by directing the attention to the variations in pitch *as such.*

A contrast in percussion sounds in many of the diseases of the chest is sufficiently obvious, requiring no special delicacy of discrimination. This is the case in pleuritis with effusion, in the second stage of pneumonitis, and in phthisis when the tubercelous deposit is abundant. Attention to pitch, under these circumstances, may be said, with truth, not to be of importance. A flat, and notably dull sound are readily enongh discovered. It is in connection with the diagnosis of the early stage of tuberculosis that the point under consideration is particularly important. Of the importance of the diagnosis of the disease in this stage it is not necessary to speak. In view of the employment of means to arrest the further progress of tuberculization, and the fair prospect, in many instances, of effecting that object, there is perhaps no end in practical medicine more desirable than to determine positively the presence of tubercle before the disease has made much progress. Any addition to our means of giving precision to the diagnosis of incipient phthisis is a valuable contribution, not only to our science, but to our art, inasmuch as the prospect of saving or prolonging life is greater in proportion as the affection is earlier recognized.

A disparity at the summit of the chest, however slight

is a sign to which great weight should be attached in deciding on the presence of tubercle. Practical auscultators generally will concur in the statement above quoted from the treatise by Dr. Bowditch, relative to this point. If, therefore, by attention to the pitch of resonance, we are better able to appreciate a slight shade of difference, or to recognize its existence with greater certainty, there can be no question concerning the service thereby rendered to physical exploration.

It is hardly necessary to say, that the usefulness of the means of early diagnosis in phthisis is to be regarded in both a positive and negative aspect. In other words, it is quite as important to be able to exclude tuberculosis, by the absence of physical signs, as to determine its existence by the presence of these signs, and, hence, improvements in physical exploration have practical relations of equal value, in either direction.

Another advantage to be derived from attention to pitch consists in the facility of developing a disparity in situations in which but little resonance can be elicited, owing to the intervention of bone, muscle, and fat between the integument and walls of the chest. A late writer on diseases of the chest says, that the mammary region in females "is of no value in percussion."* The same author farther states that, "posteriorly, no advantages can be derived from percussion; the muscles are too thick about the shoulders to admit of a decided advantage even in cases in which the pulmonary condensation is much greater than in incipient phthisis" These statements are to me surprising. A disparity of pitch is frequently very apparent in the scapular region, both above and below the spine, in cases of tuberculosis, and I have met with it here when it was not marked in the infra-clavicular region. I have demonstrated this fact repeatedly during the short period that has elapsed since the publication of the work from which the above extracts are taken. To dispense with the information from percussion sounds over the scapulæ would considerably impair the diagnostic resources in incipient phthisis. The thickness of the muscular coating in other parts of the chest, and even the

* A Treatise on Diseases of the Chest, by John A. Swett, M. D., 1852, pp. 17 and 258.

mamma of the female, do not prevent a comparison of sounds with reference to pitch; but, for reasons obvious enough to the pathologist, the comparison in these situations is less important in diognosis, save in diseases in which the coarsest exploration would suffice to develop flatness, as in pneumonitis and pleurisy.

Another advantage relates to the diagnosis of heart diseases. Percussing from the clavicle, the acromial angle, and the axilla, in a direction toward the site of the heart, the points at which there is a distinct modification of pitch will mark the limits of the organ in these directions, while the point of impulse, if perceptible, will give the lower boundary. This method, thus, will be found useful in determining the degree of enlargement in hypertrophy and dilatation, and the amount of effusion in pericarditis.

The significance of variations in pitch, as of other points of disparity, in the diagnosis of thoracic diseases, of course, is predicated on the equality of both sides of the chest, in this respect, in health. Does this equality exist? I cannot answer this question quite so explicitly as I could desire. Pitch modifications will necessarily result from causes which at the same time occasion obvious dullness, such as spinal curvatures, former pleurisies, arrested tuberculous deposit, &c. These causes, and others, moreover, may occasion, in some instances, a disparity in pitch, when the changes are not sufficient to produce obvious dullness. Comparison in pitch affording a more delicate means of discovering a slight deviation from the symmetry of the two sides, it is not improbable that healthy chests may be less uniformly equal in this particular, than with respect to a marked diminution of resonance on one side. The examination of a large number of individuals presumed to be free from pulmonary disease would alone afford the data for answering the above question positively. I have frequently examined chests, at the summit more particularly, with reference to this point, but without recording the results. Within a few days I have noted observations in twenty-two persons. In all save *three*, the pitch was equal in the infraclavicular region, to which attention was more particularly directed. In *one*, there was elevation of pitch on the right side, apparently owing to the greater develop-

ment of the muscles on this side. In another, the same disparity existed, the same explanation not being so obvious. There was no reason to suspect disease of the lungs in this latter instance. The patient was a young female in perfect health. In the third instance the percussion sounds were clear and equal at the summit anteriorly, but over the right scapula there existed evident dullness compared with the sound over the left scapula. This person was a mechanic, and the muscular mass in the interscapular space was notably thicker on the right side. It is undoubtedly desirable that examinations of this kind should be multiplied.

In practicing percussion with a view to *pitch*, observance of the rules which are familiar to every practical auscultator is peculiarly important; I allude to the position of the patient, care to strike successively on the two sides at corresponding points, and with an equal force, &c. Tact in elicting a loud distinct sound is desirable. A smart stroke is frequently more effective for this object than a light feeble blow, not, however, using sufficient force to occasion pain. The only pleximeter and precussor with which I have any practical acquaintance are the fingers. It has often occured to me that other instruments might in some instances be more satisfactory, but I have nothing to say on this topic from my own experience.

SECTION 2. *On attention to the pitch of sound in the practice of auscultation.*—My attention has been directed to the variations in the pitch of respiratory sounds more recently, than to those belonging to percussion.—It is for a few months only that I have made the former objects of study During this time, as leisure and opportunities have permitted, I have been accustomed to note observations on the different cases of thoracic diseases coming under my notice; and the views which I shall submit in this section will consist of deductions from the data thus collected. Considering the novelty of the subject, as well as its importance, I have thought it best to present, not only the conclusions based on what data I have gathered, but a transcript of the data themselves. As an appendix to this section, therefore, I shall give a brief synopsis of the observations which have been made in cases of disease, amounting to over forty in

number. A larger collection of data for generalization is undoubtedly to be desired; but it is to be borne in mind that the aim of this memoir is by no means to exhaust, but rather to open the subject, as one claiming attention, with a view to its practical application to physical diagnosis. I would here repeat that what I shall advance as the results of observations made up to this time, I wish to be considered in the light of propositions to be confirmed, enlarged, and perhaps corrected by continued investigations. As already intimated, to be able to estimate fully the liability to error of observation in a new field of study like this, may require the fruits of a longer experience. I am led to make this remark the more, in consequence of being sensible that a facility in distinguishing pitch variations in the respiratory sounds is much increased by practice. After what I have said, it is but justice to myself to add that, in making the observations I have noted, I have spared no effort to render them reliable.

The point of departure for the study of pitch modifications, as of other physical signs of disease, is the examination of the chest in health. The first inquiry, therefore, in presenting this branch of the subject, will be, *what variations in pitch belong to healthy respiration?* To this question I will devote a distinct division of s section.

Variations in the Pitch of Sounds in Healthy Respiration.—With a view to the study of the variations to be found in a healthy chest, I have noted the results of physical explorations, more or less complete, of twenty-seven individuals, presumed to be entirely free from any thoracic disease. This number of observations, although too few to settle the numerical ratio of the occurrence of particular phenomena, will probably suffice for the present object, which is merely to determine some general principles to serve as the basis of the study of morbid deviations.

Of these twenty-seven individuals twenty-one were males, and six females. The ages were various, all, with a single exception, being above childhood, and none being advanced in life. The majority were young persons from twenty to thirty years of age.

The normal respiratory sounds are resolvable into three divisions, according to the parts of pulmonary apparatus, whence they emanate, viz: the trachea, the bronchi, and the vesicles. Named from these their anatomical relations, they are the *tracheal*, the *bronchial*, and the *vesicular*. The character of pitch belonging to the sounds produced in these three situations may be considered under three distinct heads.

1. *Tracheal Sounds.*—On placing the stethoscope over the trachea, the respiratory sounds are found to be notably high in pitch. The development, that is to say the loudness of the sounds, in ordinary respiration, varies considerably in different persons. In some persons they are quite intense, in others feeble, and even indistinct until the persons are requested to breathe forcibly, when they become much increased. The relative altitude of pitch is immediately perceived on comparing the tracheal sounds with the vesicular respiration heard on listening over the chest. Two sounds are heard uniformly in this situation, viz: the sounds of inspiration and expiration. An interval occurs between these two sounds. The inspiratoty is relatively shorter than the expiratory sound. The sound of expiration is higher in pitch than that of inspiration. These results have been uniform in all the observations that I have made.

2. *Bronchial Sounds.*—Explorations for the bronchial respiration were made anteriorly near the clavicula-sternal junction, and, in a smaller portion of the cases, in the interscapular space posteriorly. Of twenty-three examinations in the former of these situations, the bronchial sound was appreciable in twenty, and not discoverable in the remaining three instances. Of fourteen examinations in the latter situation, *i. e.*, the interscapular space, it was appreciable in all but one instance. As respects the two situations, in some cases, the sounds were more developed posteriorly and anteriorly, and in other cases, the reverse. There was considerable difference in different cases in the degree of development, or intensity of the sounds. In several, no sound was discernible during ordinary respiration, but it became apparent on increasing the force and quickness of the respiratory movements.

The pitch of the bronchial sounds is high, probably no much below that of the tracheal sounds, but it did not occur to me to compare the two with reference to pitch, until I began to write on the subject. The point has not much importance. The elevation of the pitch of bronchial, compared with the vesicular murmur, is to be borne in mind. A comparison of the former, as heard in the interscapular space, and near the sternum anteriorly, with the latter as heard over other parts of the chest, showed, in every instance, a distinct and notable disparity.

Other interesting points of distinction pertain to the *bronchial sounds.*

Recollecting the relations, in size and length, of the two primary bronchi to each other, the inquiry arises if the bronchial respiration heard in corresponding situations on both sides of the chest, before and behind, is uniformly equal in intensity and pitch? With respect to intensity or loudness, of *thirteen* examinations it was thought to be somewhat more developed on the left side in four cases, on the right side in five, and in four cases no difference in this particular was apparent. As regards pitch, the result was different. Of twenty examinations the pitch was noted to have been distinctly higher on the right side in fifteen, no difference being appreciable in the remaining five cases. This disparity in pitch between the two sides was not observed in all instances, both anteriorly and posteriorly. Excluding seven instances in which it is merely noted that the pitch was higher on the right side, without specifying whether before or behind, the notations in the remainder of the cases are as follows: 1. No disparity in front, but notably higher on the right side behind. 2. Higher on the right side before and behind. 3. The same. 4 The same. 5. Higher on the right behind, where the bronchial sound is alone heard. 6. Slightly higher on the right side in front, no difference being appreciable behind. 7. Higher on the right side before and behind. 8. The same.

The existence of disparity in the bronchial sounds of the right and left side, attributable to the difference in size and length of the bronchial divisions of the trachea has been peinted out by Dr. Gerhard, of Philadelphia, but the fact has not been recognized by all auscultators. Dr. Stokes, on the contrary, has taught that a disparity fre-

quently exists, but that the bronchial character is apt to be more apparent on the left than on the right side. Dr. Gerhard speaks of the respiration being more blowing or tubular on the right side. If my observations are correct, both Dr. Gerhard and Dr. Stokes may be right, the discrepancy arising from confounding different elements which enter into the bronchial respiration. It is occasionally more developed, that is to say, louder on the left than on the right side. So far Dr. Stokes is correct, but in a very large majority of persons the pitch is higher on the right side, while this relative elevation never obtains on the left; and, as the pitch is one of the most prominent of the elements of the bronchial respiration, the foregoing results accord with the fact pointed out by Dr. Gerhard. The observations of these two distinguished writers are thus apparently not really inconsistent with each other.

In degree, or loudness, the normal bronchial respiration presents in different individuals great variations. In this point of view it has no uniformity. It is never intense, and, in some persons, is quite faint, requiring a forcible respiration to develop it sufficiently to study its characters; and it is not appreciable in all persons.

In a certain proportion of the cases in which the normal bronchial respiration is heard, not uniformly, a sound of expiration is appreciable. Of thirteen instances in which the inspiratory sound was present, a sound of expiration also existed, the latter being absent in four. The facts with respect to these variations contained in the few observations I have noted are as follows: Of the above thirteen instances, in six the sound of expiration was perceived only on the right side, either in front or behind, and in seven it was appreciable behind, and not in front.

The sound of expiration appeared to be higher in pitch than the sound of inspiration, in every instance in which attention was directed to this point. This fact is noted in nine observations, an exception thereto not being noted in any. Thus, in this trait, the normal bronchial respiration resembles the tracheal.

In every instance in which attention was directed to the succession of the sounds of inspiration and expiration, an interval between was observed. This feature belongs to the bronchial, in common with the tracheal respiration.

It will thus be noticed that, in the more important of the elements of the tracheal and bronchial sounds, they are similar. They both want a distinctive quality which will be seen to characterize the vesicular respiration. They are both high in pitch, in this respect probably not differing much from each other. The inspiratory sound in both is short. The expiration in each is higher in pitch than the inspiration; the difference with respect to this point between the tracheal and the bronchial respiration, being that in the latter an expiratory sound is heard in a certain proportion of cases only, while it is uniformly heard in the former. In each, an interval occurs between the sounds of inspiration and expiration. The characters distinguishing the two kinds of respiration consist in the greater intensity of the tracheal sounds, the uniformity with which they are heard in different persons, and the constant presence of an expiratory sound.

3. *Vesicular Respiration.*—The vesicular respiration, as is well known, differs from the tracheal and bronchial in having a peculiar quality, not capable of being very definitely expressed by language, but which is readily enough appreciated by the practiced ear. The terms *breezy* and *expansive*, perhaps, approach as near a definition as can be done by words. This quality of sound is *sui generis*, and familiarity with it is very necessary to the practical auscultator. The peculiarity alluded to may be characterized as, *par excellence*, the *vesicular quality*. I shall have occasion to refer to it hereafter by this title.

In what other particulars does the vesicular respiration differ from the two varieties already noticed?

The difference in pitch is striking. The pitch is uniformly and notably lower than that of the tracheal or bronchial respiration. This was true of all the cases examined with reference to healthy variations. As a point of difference, it is one to which, in connection with the subject of this memoir, special attention is desired.

An expiratory sound is appreciable in a less number of instances than is the case with the bronchial respiration. Of nineteen examinations with reference to this point, the expiration was heard in nine. The pitch of the expiratory sound, when it is heard, compared with that of the inspiration, is another point of special interest in the

present inquiries. Of eight observations in which the facts relating to this comparison were noted, the pitch of the expiration was lower in all but two instances. In these two instances, the pitch of expiration was higher in the right infra-clavicular region towards the sternum, no expiratory sound being appreciable, in either instance, except in the situation first mentioned. So far as these examinations go, then, the rule is, that the sound of expiration in vesicular respiration is lower in pitch than the sound of inspiration, with occasional exceptions* at the summit of the chest on the right side towards the sternum. This rule, without the exceptions, is stated by Dr. Walshe in the last edition of his work on the heart and lungs.

Aside from the foregoing differances, the duration of the inspiratory sound, in the vesicular respiration, is longer than in the tracheal or bronchial. The expiratory sound, on the other hand, is as notably shorter; and when a sound of expiration is appreciable, it is nearly or quite continuous with the sound of inspiration. The latter statement I give on the authority of others, and my own unrecorded experience, not having taken pains to note the facts relating to the point in healthy observations made with reference to the subject under consideration.† There are some other variations in pitch which are of practical interest. The pitch of vesicular respiration at the upper part of the chest is higher than at the lower. This I have noted in eleven observations, which were all that have been made relative to this point. The pitch does not appear to be sensibly raised by increasing the force of the respiratory movements. It seems to remain the same in ordinary and forced respiration. This I have noted in eight observations, being all that were made relative to

* The sound of expiration is oftener heard when the vesicular sounds are exaggerated in supplementary respiration. In the clinical observations of cases of pneumonitis, the presence of a sound of expiration on the healtny side, and the lowness of pitch compared with the sound of inspiration, are noted in several instances. See Appendix First Series, Cases of Pneumonitis.

† The continuousness of the expiration with the inspiration is noted in several of the clinical observations, in which the lungs on one side were free from disease. See Appendix, First Series. Cases of Pneumonitis.

the point The vesicular respiration has a sensibly higher pitch at the summit of the right than of the left chest, in a large proportion of individuals. Of fifteen examinations relative to this point, the disparity just noted was found to be more or less marked in eleven instances, no difference between the two sides being apparent in the remaining four. The practical bearing of the several facts stated in this paragraph on physical exploration in disease will be at once obvious.

In conclusion, while the tracheal and bronchial sounds were found to be essentially the same in character, the circumstances distinguishing each from the other being rather incidental than intrinsic, the vesicular respiration, on the other hand, contrasted with the two former, exhibits some striking points of dissimilarity. It has that inexpressible peculiarity distinguished as the vesicular quality. The inspiratory sound is lower in pitch. The expiratory sound, when heard, save in a limited situation, is lower than tne sound of inspiration, the reverse of this being true of the tracheal and bronchial sounds. Distinctive features less prominent, but important, are the greater length of the inspiratory sound, the shortness of the sound of expiration, the continuity of these two sounds, and the fact that in a large proportion of cases the sound of inspiration alone is appreciable.

Variations of Pitch in Respiratory Sounds in cases of Disease.—In treating of morbid variations, it will be most convenient to consider, under separate heads, the different diseases in which they have been studied. The divisions will then correspond with those of the clinical observations given in the Appendix to this memoir.

Pneumonitis.—The present inquiry respecting pitch variations of sound has no reference to râles. The crepitant râle in the first stage of pneumonitis frequently drowns other sounds. The modifications which relate to the present subject are incident to the solidification belonging to the second stage of the disease. More or less crepitation, as is well known, may continue into this stage, being heard at the end of the inspiratory act. Under these circumstances, enough of the respiratory sound may be heard, before it is lost in the crepitation, for the pitch to be compared with that of the respiratory sound on the healthy sid.

The observations pertaining to pneumonitis, among those which I have collected, amount to *twelve*. This number might have been increased since my attention has been directed to this subject, but it is probably sufficient for the present object. In each of these observations, the pitch of respiration was notably high, presenting a striking contrast, in this respect, with the respiration on the healthy side. The disparity, as I have noted in one of the observations, is sometimes as obvious as between a fife and German flute. Of these *twelve* observations, an expiratory sound was more or less developed in *eight* instances. In *two*, a sound of expiration was not appreciable; in *one* instance, it was appreciable, but too feeble for its characters to be studied, and in *one* instance the fact as to the presence or absence of the expiratory sound is not noted. The pitch of the sound of expiration was higher than that of inspiration in every instance in which attention was directed to this point, save *one*. In the excepted instance, the observation was made in an early stage of the disease; the crepitant râle persisting, the expiratory sound was feeble, and the fact of its being lower is stated somewhat distrustfully. The expression is, it appears to be lower in pitch. The correctness of this observation seems to me open to doubt, for, as I have had occasion to notice, there is a liability to err in estimating the pitch of a faint sound of respiration. Its feebleness may, without due attention, give rise to the impression that it is lower in pitch. With this doubtful exception, the elevation of the pitch of expiration over that of inspiration was uniform, wherever the expiratory sound was sufficiently appreciable to study its characters.

Aside from what relates to the above pitch modifications, the facts noted with respect to other characters are as follows: The inspiration was shortened in every instance in which attention was directed to this point. This is noted in *five* observations. An interval was observable between the sounds of inspiration and expiration in all the observations containing information on this point: viz., in *six*. The expiration was prolonged whenever attention was directed to this point. It is noted in *five* observations.

The characters, then, belonging to the respiratory sounds incident to pulmonary solidification in pneumonitis, are, elevation of the pitch of both inspiration and expi-

ration; a higher pitch of expiration than of inspiration, whenever the former is heard, almost if not quite uniformly; a shortened inspiratory sound; a prolonged sound of expiration; and an interval between the sounds of inspiration and expiration.

The respiration in the second stage of pneumonitis, as is well known is sometimes characterized as *tubular*, and sometimes as *bronchial*. As contrasted with the vesicular respiration, the first term is certainly significant. The sound may also, with great propriety, be called bronchial, for, on comparing the several points just mentioned with the different elements entering into the normal tracheal and bronchial sounds, more especially the latter, it is perceived that they are essentially identical. The bronchial respiration in pneumonitis is neither more nor less than the bronchial respiration heard in the healthy chest in certain situations. One of the chief points of difference between the normal bronchial and treacheal sounds, it has been seen, is the greater intensity of the latter. The respiratory sounds in pneumonitis vary considerably, in different cases, in degree. The intensity appears to be a contingent, rather than an intrinsic element. In some instances, the development of sound is fully equal to that heard over the trachea, and it may be even louder than is the treachel respiration in some individuals. This comparison of the respiration in pneumonitis with the tracheal and bronchial sounds would naturally enough lead to inquiries respecting the physical philosophy of the identity in their characters. Such inquiries, however, are entirely foreign to my present purpose. To contrast the respiration of pneumonitis with the normal vesicular murmur would be to repeat the points of difference already noticed in connection with the variations incident to health. The disparity is, of course, based on the same circumstances as that between the tracheal and bronchial, and the vesicular sounds.

Pleuritis.—The observations relating to pleuritis were made but in *three* cases. So far as these cases go, they show that, when considerable effusion exists, the sound over the compressed lung is high in pitch, presenting, also, more or less of the other characters belonging to the bronchial respiration. After the fluid has been removed nearly, or quite, by absorption, more or less pleuritic ad-

hesions limiting the expansibility of the affected side, the respiratory sound, having resumed the vesicular quality, is relatively feeble when compared with the sound on the other side which is exaggerated, but a disparity in pitch may not be appreciable. If, however, the quantity of effusion has been large, so as to have distended greatly the chest, and led to considerable contraction after the absorption is effected, then, in connection with some permanent dullness on percussion over the affected side, the pitch of respiration may be higher than on the other side, although the vesicular quality and other characters of the vesicular respiration are resumed.

For the exemplification of these statements the reader is referred to the Appendix, Second Series of Observations.

Gangrenous Excavation, and pneumothorax with Perforation.—I have had an opportunity of observing, since my attention has been directed to this subject, the respiratory sounds in but one instance of gangrenous excavation, and one of pneumothorax, both occurring in the same case.

Over, or near the site of the gangrenous excavation, which was considerable in size, the sound was noted to be non-vesicular, and low in pitch; as respects the latter, presenting a striking contrast with the sound heard at the summit of the chest, in the same case, where the lung was solidified by compression. Over the latter, the sound was equally non-vesicular, but high in pitch.

The entrance of air into the pleural cavity through a perforation of about the size of a goose-quill occasioned a sound, non-vesicular, and low in pitch.

For a brief account of the case, together with the autopsical appearances, the reader is referred to the Appendix.

Tuberculosis.—The physical signs incident to tuberculosis have relation not merely to the presence of the morbid deposit, but to its amount, and the stage of the affection. The study of the disease, with reference to the principles of diagnosis, thus presents itself under somewhat different aspects. During the past two months, I have recorded observations, with respect to variations in the pitch of respiration, in *twenty-five* cases of tuberculosis. I propose to examine the results of these observations under four heads, conforming to the distribution for

the cases into four groups in the Appendix to this memoir. Following this arrangement, I shall consider, *first*, the observations in cases in which the tuberculous deposit was supposed to be small in amount; *second*, when the quantity of the deposit was comparatively abundant; *third*, when the disease had proceeded to the stage of excavation; and *fourth*, when the affection appeared to have been arrested.

I. *Observations in Cases of Small Tuberculous Deposit.* —*Eleven* of the cases fall under this subdivision. In each of these cases, the pitch of respiration was found to be higher over the site of the tuberculous deposit, as determined by relative dullness on percussion. In order to test the reality of this variation, and ascertain whether the perceptions might not be influeneed by preconceptions, in several of the cases included in this and the next subdivision, ausculation with reference to the pitch was practiced prior to percussion, or any other examination of the chest. In every instance, the pitch modification indicated the side on which relative dullness on percussion was afterward found to exist. I have frequently caused this experiment to be made by several young gentlemen, medical students, who have accompanied me in my hospital visits, and with uniform success. That is to say, the difference in the pitch of respiration furnished a ready criterion of the existence, or greater abundance of the deposit on one side, before resorting to percussion or other signs.

In some of the observations, the mere fact of elevation of pitch on one side is stated. I was at first satisfied with simply ascertaining this fact, without attending particularly to the expiration, or other points. In several of the cases, however, the observations were more comprehensive.

It is noted that the sound of expiration was appreciable in *six* cases, and inappreciable in *two;* in *three* of the observations, nothing being stated on this point. The expiration is stated to have been prolonged in *four* cases. In *five* cases, the expiration was thought to be either equal to, or higher in pitch than the inspiration: or, to be exact, in *three* of these five cases it was thought to be higher; in the other *two*, it is stated to have been as high, if not higher.

In some cases the vesicular quality was obviously impaired, in connection with the elevation of pitch; but in other cases, disparity in this respect was not obvious. So, with regard to development or intensity of sound, it was sometimes diminished, but not invariably.

To determine the presence of a small amount of tuberculous deposit, in other words, the diagnosis of incipient phthisis, is frequently one of the most difficult problems in practical medicine. Certainly, the employment of physical exploration with reference to this point requires as much accuracy, care and skill as in any of its applications. Several of the auscultatory signs which have more or less importance as indicating the presence of tubercle, are only occasionally present, and the evidence furnished by them is circumstantial, rather than positive. This remark applies to the signs denoting circumscribed bronchitis, pneumonitis, or pleurisy, in proximity to the morbid deposit; the sibilant or mucous râles at the summit of the chest; the crepitant râle, or friction sound, heard in the same situation. Other signs which are doubtless of importance in this connection, must be regarded as somewhat equivocal, such as the jerking respiration, and a prolonged expiration. The most direct and constant of the signs incident to a small tuberculous deposit is the modification known as the *rude*, sometimes called the *harsh* or *rough* respiration. This modification has relation to the present subject, while, to consider the other signs referred to, in this connection, would be irrelevant.

For the novice in the study and practice of physical exploration, the *rude* respiration is generally, of all the signs, the one most difficult to be apprehended. There is probably no sign which causes the teacher or writer more embarrassment in his efforts to explain clearly. Take, for example, the description by Dr. Hughes, author of a very lucid treatise on physical diagnosis. Speaking of the *rude*, compared with the *vesicular* respiration, he says, it "is the *forte* of the same note, but on a loose and jarring string;" and, again, contrasting it with the normal vesicular murmur, he says, "In the one, the same soft breeze passes through a greater number of trees; in the other, the breeze is increased to a moderate gale." Not only is there a singular indefiniteness in the idea conveyed by this language, but the analogies selected for il-

lustration are defective in correctness. The *note* is not the same in *rude*, as in normal respiration, and the comparison to the gale is calculated to give the erroneous impression that the rude respiration is necessarily louder than the normal, which is so far from being true, that it may be in a notable degree less developed; the intensity being a variable element, having nothing to do with the distinctive character of the sign. The truth is, these terms *rude*, *rough*, and *harsh*, are unfortunate; they are not only inexpressive, but tend rather to mislead in the apprehension of the modification referred to. The sound is not intrinsically *rude*, or *rough*, or *harsh*. Even a well-marked bronchial respiration, to which the sound under consideration is an approximation, can hardly be said to have the qualities indicated by these terms. The bronchial respiration is not very unlike in character the endocardial sound. which is characterized as a *soft* bellows murmur. Whatever appropriateness the designation has, is based chiefly on the fact that, in the rude respiration, the peculiar expansive, breezy attribute of the vesicular respiration, which has beeen referred to as the *vesicular quality*, is more or less impaired.

To form a correct idea of the modification usually termed *rude*, &c., it must be analytically decomposed, and the nature of its elements determined. It is an approximation to the bronchial respiration. It exhibits an incipient development of the character distinguishing the bronchial from the vesicular respiration. One of the most striking of these characters is the change in pitch. The pitch is raised. The vesicular quality is diminished; hence it approaches to a tubular or blowing respiration. The inspiration may be somewhat shortened, and occasionally a sound of expiration becomes developed and prolonged, constituting an important rythmical variation. By attention to these several points much will be gained in practically recognizing the modification; but it is not easy to find a satisfactory title to be substituted for the names generally in use, Of the several elements mentioned here, as in the case of the bronchial respiration, it seems to me the pitch modification is the most striking, and the most readily appreciated, while it is probably the most constant.

The expiration deserves to be distinctly noticed. Con-

siderable importance has been attached to a prolonged expiration, since its occurrence was pointed out as a frequent sign of early tuberculization, by James Jackson, the younger, of Boston, Mass. The best practical authorities have recognized this sign as a valuable contribution to the art of physical exploration. It is not, however, a constant modification. Its absence, therefore, does not furnish ground for the conclusion that tubercle does *not* exist. Moreover, it may exist as a normal peculiarity, and consequently alone it is not perfectly reliable. In the few instances among the cases I have collected in which a prolonged expiration was present, and the pitch noted, it was found to be higher, or as high as the sound of inspiration. Now, in health, over the greater part of the chest, if a sound of expiration be appreciable, it is found to be distinctly lower in pitch than the sound of inspiration. May it not be that the elevation of pitch in expiration has a diagnostic value fully as great, or even greater than when the inspiration is thus modified? May it not be that, in some cases in which the inspiration is not sensibly raised in pitch, the expiration may exhibit a change in this respect, and thus the latter furnish a more delicate sign of the presence of the disease? These are interesting, and possibly important questions, which are to be satisfactorily answered only by an accumulation of observations.

It occurs to me here to remark (what would have been more appropriate in connection with pneumonitis,) that the observations of Jackson and others have showed the expiration to become earliest, and in the most striking degree changed, in the development of the bronchial respiration. This fact would lead to the presumption that, in the modification generally known as the rude respiration, the change would be first and most distinctly declared, in the expiration. The expiration in the development of bronchial respiration is said first to exhibit the bronchial character. Although not hitherto recognized as such, I can have little doubt that the change thus noticed consists chiefly in the elevation of pitch. This produces a more striking change in the expiration than in the inspiration, because the change is really greater in amount. The expiration in health is lower in pitch, while in the bronchial it is higher than the inspiration. The degree of modification is, therefore, greater than in the case of the inspiratory sound.

In conclusion, it is highly important to bear in mind that the pitch of respiration at the summit of the right chest is frequently higher than on the left side, and that this may, or may not coexist with a greater development and prolongation of the sound of expiration on the right side. The practical inference is that these modifications, when present on the right side, are, in themselves, less significant than when they occur on the left side, and, in the former situation, are to be less confidently relied upon, especially in the absence of marked rational evidences of tuberculous disease.

II. *Observations in Cases in which the Tuberculous Deposit was abundant.*—The cases arranged under this head are comparatively few in number, amounting to but *five.* In each of these cases, on the side in which the tuberculous deposit was most abundant, as declared by the relative dullness on percussion, and other signs, the respiratory sound was found to be higher in pitch than on the side in which the morbid product was less in amount. In *three* of the cases, the disparity in the pitch of respiration is simply noted in general terms. In the fourth observation it is stated that there existed a prolonged expiration, with an interval between the sounds of inspiration and expiration. The expiratory sound, however, was so feeble that its pitch was not ascertained.

In the fifth observation, an expiratory sound is stated to have been present, higher in pitch than the inspiration, with an interval between the two sounds.

So far as these observations go they lead to the conclusion that the respiratory sound over the site of an abundant tuberculous deposit, when it is appreciable or not obscured by râles, will be found to be elevated in pitch, and representing more or less of the other characters distinguishing the bronchial respiration.

III. *Observations in Cases of Tubercle advanced to the Stage of Excavation.*—The cases coming under this head are of special interest, having reference to the study of the variations in the pitch of respiration occasioned by the presence of a cavity or of cavities within the chest. It has been seen already, in the single case of perforation of the lung and gangrenous cavity, presented in another division, that cavernous respiration, under these circumstances, was low in pitch. This was true in all the cases

of tuberculous excavation embraced in this subdivision. The cases studied with reference to the cavernous respiration are *seven* in number. In one of these cases, however, the existence of cavities which had been predicated on physical signs, was disproved by post mortem examination. This case is included in the series for convenience, and serves to illustrate certain points in diagnosis which are to be carefully attended to, in order to avoid an erroneous conclusion. Of the remaining *six* cases, in *two*, autopsical examinations were made, showing the existence of cavities, and their situation with reference to the parts of the chest where the physical signs of excavation had been determined, as noted in the recorded observation before death. The dissection in both instances was made at my request, by Dr. John C. Dalton, Jr., to whom I am indebted for reports of the morbid appearances. The attention of the reader is particularly invited to the cases which include autopsies. (See observations 1 and 3, under same head in Appendix.) In the *four* cases without autopsies, the existence of excavations and their particular situations, are not, of course, demonstrated. The evidence consists of the physical signs, and other facts pertaining to the histories.

So far as the observations go, they show, as already remarked, that the pitch of the cavernous respiration is low, contrasting in this respect, strongly with the high-pitched bronchial respiration. This is not claimed as a newly discovered fact. It is stated by Dr. Walshe, in the last edition of his treatise on diseases of the lungs and heart; and is implied in the language used by Barth and Roger. It is not, however, dwelt upon by either with much emphasis, and, if I am not mistaken, it does not generally receive, with practical auscultators, attention as a diagnostic criterion.*

The pitch of the expiration, when it enters into the cavernous respiration, does not appear to have attracted notice. In the cases I have observed, in which the sound of expiration was appreciable, the pitch was lower than that of inspiration. This forms a striking feature distin-

* For example, in a treatise on disease of the chest, issued during the present year, I find the following statement: "A naturally cavernous respiration exists over the trachea and larynx."

guishing the cavernous from the bronchial respiration. In the latter, as has been seen, the sound of expiration, when appreciable, is higher, or at least equal in pitch to the sound of inspiration.

The cavernous sound is, of course, devoid of what has frequently been referred to as the vesicular quality of normal respiration. This point is generally noted in the observations. It is a point important to be borne in mind in determining the presence of a cavernous respiration; else the vesicular murmur, with its low pitch of inspiration, followed by a still lower expiration, may be thought to denote an excavation. Obs. 7 (see Appendix) illustrates the liability to error incident, in part, to not observing sufficiently this precaution.

The following elements, then, combine to form the cavernous respiration: A *blowing* sound, that is to say, a sound in which the vesicular quality is absent; a low pitch compared with that of the bronchial sound; an expiration, if present, lower in pitch than the inspiration.

Keeping in view these characters, it is probably easy, in most instances in which there are cavities of any considerable size, to determine, with precision, their particular situations, by ascertaining the limits circumscribing the space over which a well-marked cavernous respiration is found to be present. We are assisted in defining the boundaries of these spaces by the fact that excavations are generally surrounded by tuberculous consolidation which will be likely to raise the pitch, and thus exhibit the cavernous sound in stronger contrast. If, however, this were not true in any instance, but, on the contrary, supposing a cavity to be surrounded with lung giving a vesicular sound, we can generally satisfy ourselves of the lowness of pitch (having already satisfied ourselves of the non-vesicular character of the sound in question,) by comparing it with the normal bronchial respiration heard at the sterno-clavicular junction, or with the tracheal respiration. A cavity at the apex of the lung will be more readily discovered by the respiration if it be anteriorly and superficially situated. It is hardly necessary to add that it must be more or less empty, and communicate more or less freely with the bronchial tubes. To ascertain the pitch and other characters, also, the respiration in the surrounding lung must be free from râles.

Tuberculous excavations, as is well known, generally coexist with a greater or less amount of crude tuberculous deposit. In cases, therefore, which present the physical signs of cavities, we have a high-pitched non-vesicular sound, *i. e.*, the bronchial respiration, more or less, over parts of the chest, at its summit, in which the cavernous variations are absent. The observations in the cases of tubercle advanced to excavation furnish illustrations of this fact.

In searching for the sites of cavities, the stethoscope is obviously preferable, since it enables us to circumscribe better the source of sounds. In immediate auscultation, the sounds are received from a wider circuit. The charter of the respiration, as heard by the latter method, will depend on the predominance of excavations or solidification within a certain distance of the part to which the ear is applied. If a large cavity exists, or several cavities, although the intervening tissue be solidified so as to give dullness on percussion, the pitch of respiration may be lower than on the other side in which the tuberculous disease is less advanced. There, the combination of a low, non-vesicular respiration, with dullness on percussion, affords presumptive evidence of the stage of excavation. This is illustrated by Obs. 1. See Appendix.

The importance of attention to the pitch variations in determining the presence of the cavernous respiration is enhanced by the fact that other physical signs of excavations are inconstant and unreliable. A tympanitic resonance on percussion cannot be depended on. It may be absent, although cavities exist, and may be present without excavations. The cracked pot variety is rarely discoverable, and may be due to the expulsion of air from the bronchial tubes or other morbid conditions. Pectoriloquy, which was regarded by Laennec as a pathognomonic sign, is not only frequently absent, but does not possess that distinctive significance attributed to it by the illustrious founder of auscultation. Between pectoriloquy and intense bronchophony there is no intrinsic difference. In other words, pectoriloquy is but a variety of broncophony, and may occur not only in cases with excavations, but when the lung between a bronchial tube and the ear is in a state of solidification from tubercle or effused fibrin. For this statement I have the authority of

Dr. Walshe. But, if it be substantiated by a sufficient number of observations that a cavernous respiration is uniformly low in pitch, with a sound of expiration lower than the inspiration, these traits, taken in connection with its blowing or non-vesicular character, will suffice for its recognition, and consequently, not only for the diagnosis of the stage of excavation, but the localization of the cavity, the estimation of size, &c.

IV. *Arrested Tubercle.*—During the time I have been engaged in making observations with reference to the present subject, two cases have fallen under my notice in which the tuberculous disease appeared to have been arrested; in other words, the morbid product had not continued to accumulate, nor had the primary deposit advanced through the processes of softening and excavation. The cases have not any special importance in connection with the subject of this memoir. I have given them under a distinct head in the Appendix, because they are distinguished from the other cases by the fact of the disease having ceased to make progress.

In this point of view the cases are not without interest. In the first case the disease was declared a year previous, and some cough and expectoration still remain. In the second case, the rational symptoms marking the period of the tuberculous deposit occurred several years ago, and the patient is entirely free from any symptoms of pulmonary disorder. In both instances the disease has left permanent evidence of slight injury to the lung on one side, consisting of disparity of resonance on percussion, and an elevation of the pitch of respiration.

The arrest of tubercular disease is doubtless much more frequent than has been heretofore recognized; and with the views now entertained by the most intelligent practitioners respecting the pathology of tubercle, and the proper ends of treatment, there is reason to hope that cases of this kind will become more numerous. As one of the causes of a lasting disparity between the two sides of the chest in the physical signs developed by percussion and auscultation, this is to be borne in mind in the examination of patients with reference to existing pulmonary affections.

In the practice of auscultation with the view to pitch variations, the same rule of course obtains which is ap-

plicable to physical exploration in general, viz: the sounds of the two sides of the chest, in corresponding situations, are to be listened to in succession, and compared with each other, always recollecting to make due allowance for certain natural deviations which are determined by observations made on persons in health, and also, as just remarked, bearing in mind the changes incident to pre-existing disease. That the respiratory sounds may present strongly marked variations in pitch will be readily understood, if it be observed for a moment how easy it is to breathe audibly with the mouth in unison with musical notes, and even in this way to hum a tune. Almost any one who has any fondness* for music is practically familiar with this. Or, as an illustration, let the pitch of sound be observed when different words or letters are whispered after the ingenious method of representing the variations in pitch of the bellows murmur of the heart, suggested by Bouillaud and Hope. The letters *R. S.*, and the syllable *who*, thus exhibit quite different degrees of altitude in pitch. It is easy to distinguish the difference in pitch among endocardial sounds. There is no greater difficulty in appreciating variations in this respect in pulmonary sounds. A musical ear is doubtless an advantage, as also some musical cultivation; but neither is absolutely essential. A medical student, who has been accustomed to follow me in my examinations, and who is unable to distinguish one tune from another, finds no difficulty in recognizing the pitch variations in respiration, and in several instances has made and noted observations by himself which have corresponded with mine. The practice of comparing the pitch of respiratory sounds doubtless leads to an increased facility in directing the attention to, and clearly perceiving variations. This I have found in my short experience, since I have been particularly interested in the subject.

In conclusion, the more important practical deductions submitted in this section are recapitulated in the following summary. These deductions, I would again repeat, are

* The pitch of respiratory sound may be readily imitated by modulating, with the lips, breath sounds in the mouth. I have sometimes found this useful in comparing the pitch of sound in the two sides of the chest.

submitted as propositions to be confirmed, enlarged, or corrected by further investigations:

1. In the second stage of pneumonitis, the inspiratory sound is high in pitch, followed by an expiratory sound, which is frequently, if not generally higher in pitch than the sound of inspiration; these traits being found in conjunction with more or less of the other characters which belong to the bronchial respiration.

2. In cases of small tuberculous deposit, or incipient phthisis, the most striking modification of the respiratory sound is the elevation of pitch. This elevation of pitch is an important element of what is generally known as the *rude*, *rough*, or *harsh* respiration. If an expiratory sound be appreciable under these circumstances, it may be as high, or higher in pitch, than the sound of inspiration, and the variation of pitch in the former is greater, inasmuch as the pitch of expiration in the normal murmur is lower than that of inspiration. Elevation of the pitch of expiration, therefore, may be found to be valuable as a sign of incipient phthisis in some cases in which the variation in inspiration is not marked.

3. If the tuberculous deposit be more abundant, the pitch of respiration is in a more marked degree elevated. The expiratory sound, if appreciable, will be more likely to be as high, or higher in pitch than the sound of inspiration. More or less of the other characters of the bronchial respiration are at the same time present.

4. In pleurisy with effusion, the pitch of respiratory sound is elevated, in conjunction with more or less the characters of the bronchial respiration, over the parts of the chest lying above the compressed lung. In cases of large effusion, after its complete removal by absorption, the affected side may continue to present a variation in pitch, the symmetry of the two sides being permanently impaired, in this respect, after the vesicular quality of respiration is regained.

5. In cases in which tubercle has advanced to the stage of excavation, the site of a cavity of considerable size is indicated by a blowing sound, low in pitch with an expiratory sound (if appreciable) lower in pitch than the sound of inspiration. These traits constitute the elements of the cavernous respiration, and the cavernous respiration is the most constant and reliable of the signs of an excavation.

If the cavity be very large, or there are several cavities, the respiration may be modified to such an extent that, on immediate auscultation, over the whole summit of the chest, it may present the cavernous characters. This may be the case while dullness on percussion shows the existence of more or less solidification in connection with the cavities. The coexistence of relative dullness on percussion, and a low-pitched blowing respiration, denotes the predominance of excavation.

The cavernous respiration may also be present in cases of excavation from circumscribed gangrene, and in pneumothorax with perforation.

6. In arrested phthisis, the traces of the disease may be manifested by a permanent variation in the pitch of respiration, in connection with more or less dullness on percussion at the summit of the chest on either side.

APPENDIX.—CLINICAL OBSERVATIONS.

The following account of clinical observations embraces a synopsis of the characters of respiratory sounds relating to the subject of the foregoing essay, as noted at the time the examinations in the cases severally were made. Aside from these, details pertaining to the histories of the cases will be introduced, only so far as they seem to be important with reference to the diagnosis, the stage of the disease, or the appearances found after death. The object will be to condense as much as possible, with due regard to the points just mentioned.

For convenience of reference, the observations are distributed into different nosological groups; cases of each disease forming a distinct series. The classification of the observations corresponds with the arrangement of subjects in the foregoing essay, so that reference from either to the other may be easily made.

FIRST SERIES.—PNEUMONITIS.

Observation 1.—Dec. 30, 1851. Hospital patient. Pneumonitis affecting lower lobe of right lung. Second stage of the disease. Loud tubular respiration over the site of the solidified lung. Pitch of respiration notably high.

Obs. 2.—January 27, 1852. Wm. Hasmer. Hospital patient. Disease of seven days' standing. Flatness on percussion over the lower third of chest, posteriorly, on the left side. The respiratory sound is bronchial or tubular, the inspiration somewhat shortened; a prolonged expiration is present, with an interval between the sounds of inspiration and expiration. Over the inferior third of the

right side, posteriorly, the respiration is vesicular, supplementary, without any sound of expiration. The pitch of sound on the left side, compared with the right, is in a marked degree high, the contrast nearly as striking as between the sound of the fife and the german flute.

Obs. 3.—Jan. 29. George Young. Hospital patient. Has been confined to bed four days. Dullness on percussion exists over the inferior third of the left chest. Over this portion the respiratory sound is notably higher in pitch than over the corresponding region of the right side. The inspiration is somewhat shortened. There is a faint but prolonged expiration, and an interval between the two sounds.

Obs. 4.—Jan. 30. Mary Gaball. Hospital patient. Disease occurring as complication of continued fever, third day after taking to the bed. Pneumonitis confined to lower lobe of the right lung. Inferior third of right chest, posteriorly, flat on percussion. The pitch of respiration over this region is notably higher than on the corresponding region of the left side. The expiration is somewhat prolonged, and higher in pitch than the inspiration. A short interval between inspiration and expiration.

Obs. 5.—Wm. Wrick. Hospital patient. Disease affecting upper and middle lobes of the right lung. Respiration on the right side, posteriorly, bronchial; the pitch high, inspiration shortened, and the expiration prolonged. The pitch of the expiration is notably higher than that of inspiration. Anteriorly, the respiratory sounds on the right side too feeble to determine the pitch, &c.

On the 9th of Feb., the respiratory sound on the right side anteriorly, sufficiently developed to study characters. It is bronchial, higher in pitch than on the left side; expiration prolonged; inspiration shortened, and an interval between the two sounds. The pitch is distinctly higher than on the left side. On the left side the respiration is supplementary. A faint sound of expiration is appreciable, continuous with the sound of inspiration, and apparently lower in pitch than the inspiratory sound.

Obs. 6.—Feb. 10. Catherine Finn. Hospital patient. Disease affecting middle lobe of right lung. In first stage. Crepitant rale in mammary and axillary regions. The crepitation obscures the recognition of pitch. On

the left side the respiration is supplementary. An expiratory sound is present on this side, continuous with the sound of inspiration, and notably lower in pitch than the inspiratory sound.

Feb. 11. Crepitant rale still heard in mammary region of right side. Posteriorly, over the middle third of the right side, there exists moderate relative dulness on percussion. A feeble crepitant rale is heard in this region, but not enough to obscure the pitch of respiration, which is notably higher than over the corresponding region on the left side. The expiratory sound over the middle third of the right chest, posteriorly, is feeble, and appears to be lower in pitch than the sound of inspiration.

Obs. 7.—Feb. 12. Michael Russell. Hospital patient. Eighth day of disease. Posteriorly, over middle and lower thirds of right chest, flatness on percussion. In these regions respiration notably higher in pitch than over the corresponding situation on the left side. The sound of respiration is too feeble to determine the pitch.

Obs. 8.—Feb. 12. Philip ———. Hospital patient. Disease of a week's standing. Marked dulness on percussion over the lower and right middle thirds posteriorly. Respiratory sound in these regions, in ordinary respiration scarcely appreciable; on forcible respiration more developed, without an appreciable sound of expiration. The inspiration is higher in pitch than on the left side.

On the left side the inspiration is followed by a continuous sound of expiration, which is lower in pitch than the sound of inspiration.

Obs. 9.—Feb. 12. James Whalen. Hospital patient. Disease affecting the upper and middle lobes of right lung, of nine days' standing. Flatness on percussion exists over the upper and middle thirds anteriorly, and over the whole side posteriorly. Percussion over the lower third anteriorly, on the right side, elicits clear resonance. Anteriorly, over the upper and middle thirds, a crepitant rale is heard at the end of the inspiratory act.

Over the right side, posteriorly, the respiratory sound is notably high in pitch, and quite intense over the whole side. The sound of expiration is prolonged to an equality in duration, with the inspiration, and there is an interval between the two sounds. The expiratory sound is higher in pitch than the inspiratory. This is the more rmaked the higher up the chest the ear is applied.

Obs. 10.—Feb. 13. Jane McNolty. Hospital patient. Disease occurring as a complication of typhus, having taken to bed six days ago. Marked relative dulness on percussion exists over the middle third of left side, posteriorly, and in the axillary region of same side. Over the inferior third, anteriorly and posteriorly, a gastric tympanitic resonance is transmitted. The crepitant rale is heard in the axillary region at the end of the inspiration. Over the middle third, the inspiratory sound is moderately loud, with no appreciable sound of expiration. It is somewhat shorter than the sound of inspiration over the corresponding region of the right side. It has less of the vesicular quality, and is notably higher in pitch. In the latter respect the disparity between the two sides is most marked. A faint crepitant rale is heard at the inferior portion of the right chest posteriorly.

Obs. 11.—Feb. 13. John Jarry. Disease of a week's standing. Marked relative dulness on percussion exists over the lower third of right chest posteriorly, and distinct broncophony. No rales observed. The respiratory sounds on both sides quite feeble. Over the lower third of the right side the pitch is notably higher than over the corresponding region of the left side. It is notably higher than over the superior portion of the chest on the same side. The inspiratory sound over the lower third of the right side is relatively shorter in duration. There is a sound of expiration obviously higher in pitch than the sound of inspiration, with an interval between the two sounds. On the left side there is a short, continuous sound of of expiration, which is obviously lower in pitch than the sound of inspiration.

Remarks.—In this case, two medical students,* who had been present at previous examinations of patients, and were accustomed to verify the results of my observations, were requested to explore and compare the relative pitch of respiration on the two sides, and the relative pitch of the inspiration and expiration on both sides. Without being acquainted with the result of each other's exploration, or of mine, the conclusions in the three instances were found to be the same. The foregoing statement is noted in connection with the observation.

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* Mr. J. R. Smith and Mr. Charles Ap. A. Bowen.

Obs. 12.—Feb 19. Fitz Morris. Hospital patient. Fourth day of disease. Disease seated in the lower lobes of right lung. Crepitant rale was apparent yesterday, and is now heard at lower part of chest posteriorly. Broncophony over these regions. The respiratory sound is loud, non-vesicular, on high pitch compared with the sound on the left side. An expiratory sound is heard, higher in pitch than the sound of inspiration, with an interval between the two sounds. On the left side, no sound of expiration is appreciable.

SECOND SERIES.—PLEURITIS.

This series will embrace but three cases; in two of which the patients were laboring under the subacute form of the disease, with in one case considerable, and in the other large effusion. In the remaining case, the patient had recovered from chronic pleurisy, and the variations were, therefore, those incident to the permanent changes left by the disease.

Obs. 1.—Jan. 4. Conrad Reushling. Hospital patient. Chronic pleuritis affecting the left side. The lower and middle thirds of this side, anteriorly, are flat, and the upper third relatively dull on percussion. Posteriorly, lower third flat, and the upper and middle thirds dull. Over the upper and middle thirds posteriorly, respiration is feeble, and no sound of expiration appreciable. Over the right side the respiration is supplementary. The pitch of sound on the left side, posteriorly, is notably high. This aside from feebleness, is the most striking point of disparity on comparing with the left side. Anteriorly, over the right chest, no sound of respiration appreciable, except just below clavicle, and here the pitch was obscured by dry crackling.

Feb. 1. Up to this date he progressively improved, and had become sufficiently restored to be discharged from the hospital. On the left side, at the summit, there is now slight dulness on percussion; at the inferior portion the dulness is greater. The left chest is one-half an inch less in circumference than the right. The left shoulder is somewhat depressed. The left side expands less at the summit than the right. The respiratory sound on the right side, is exaggerated, supplementary. On the left it

is relatively feeble. In both sides, the respiratory sound has the vesicular quality, a feeble sound of expiration being appreciable, and the duration of the respiratory sounds being apparently equal. After careful examination, I cannot satisfy myself of a disparity of pitch between the two sides.

Remarks.—The following remarks were appended at the time of the last observation. This case affords an opportunity of comparing an exaggerated, with a relatively diminished vesicular sound. The physical and rational signs show that the patient has labored under subacute pleurisy, from which he has nearly recovered. There is slight condensation of the left lung from compression, and less expansibility from pleuritic adhesions. Here are two opposing circumstances as respects their probable influence on the respiration. The defective expansibility might be expected to occasion a pitch of sound somewhat lower than that on the right side, the respiration in the latter being, at the same time, exaggerated. On the other hand, the condensation would be expected to elevate the pitch. These apparently conflicting circumstances serve to antagonize each other, the fact being that the respiratory sound at the summit of the chest on both sides is not far from equal. It may be inferred from this case that exaggeration of the vesicular murmur does not tend, in a marked degree, to elevate, nor, on the other hand, a feeble vesicular murmur to lower, the pitch of sound.

Obs. 2.—Feb. 8. Ortmann. Hospital patient. The effusion, in this case, was large, compressing the lung into a small space at the upper and posterior part of the chest. The disease was in the right side. No respiratory sound is to be heard save in the clavicular, post-clavicular, supra-spinous, and interscapular regions. Over the supra-spinous, and interscapular regions, the sound is feeble, non-vesicular; the expiration louder and higher in pitch than the inspiration. The inspiration is shortened, the expiration prolonged, with an interval between the two sounds.

On the left side, in the corresponding regions, the respiratory sound is supplementary. A sound of expiration is heard, which is about one-fourth the length of the in-

spiration. The sound of expiration follows that of the inspiration without any appreciable interval. In pitch, the expiration on this side is distictly lower than the inspiration.

In pitch, the inspiration, as well as expiration, is higher on the right than on the left side; but this is more especially marked in the expiration.

Obs. 3.—Feb. 10. Eliza Moore. This patient, eighteen months ago, had subacute pleurisy of the left side. The chest, on this side, was greatly distended, the heart being dislocated, so that its pulsations were seen and felt to the right of the sternum. She has remained in hospital up to this present time, but is now quite well, and has been employed as a domestic, performing hard labor, such as washing, scrubbing floors, &c. The left chest is considerably contracted. The impulse of the heart is now felt somewhat above the normal situation.

The left chest is relatively dull on percussion. The respiratory sound has the vesicular quality, but is higher in pitch than on the right side. There is a short, feeble, continuous sound of expiration. The pitch of the expiration appears to be lower than that of the inspiration. Near the right sterno-clavicular junction, the respiration is bronchial; the inspiration high in pitch, with an expiration higher than the inspiration. It is more intense, and on a higher pitch on the right than on the left side, near the sterno-clavicular junction; this being the reverse of the vesicular respiration, which is higher in pitch on the left side.

The pitch of the vesicular respiration is not increased by a deeper and stronger act of respiration.

THIRD SERIES.—GANGRENE AND PNEUMOTHORAX.

Under this head, a single case only will be embraced. This case occurred in private practice. The patient, aged about forty-five, had suffered, for between one and two years, with occasional attacks of epilepsy. He had for many years been addicted to the use of stimulants. The mental faculties were somewhat impaired, the memory especially. On the 28th of January, after a severe epileptic paroxysm, pulmonary symptoms became developed. The physical signs of pneumonitis and pleurisy being absent the pulmonary affection was supposed to be

bronchitis. On the 5th February, when I again visited in consultation, there were the physical evidences of solidification of lung at the middle third of the right chest anteriorly and posteriorly, and a dirty expectoration with a characteristic fetid odor. It was evident that the patient was laboring under pulmonary gangrene. On the 8th, I again saw the case, and at this time the fetid expectoration had ceased, and he presented the physical signs of pneumothorax, viz: tympanitic resonance at the upper and middle thirds of the chest anteriorly, and metallic tinkling at the inferior third, just below the nipple. On the 9th, I noted as follows: "The physical exploration was brief, owing to the great prostration of the patient. On the right side, over the upper and middle thirds, anteriorly, and on the upper third, posteriorly, percussion gives a tympantic resonance. Below, flatness on percussion. The respiration over mammary region is non-vesicular, and low in pitch. Above this region, in front, the sound is indistinct. On the upper third, posteriorly, the respiration is high in pitch. Below, feeble, but low in pitch."

The patient died on the 10th, and the chest was examined on the 13th, by Dr. John C. Dalton, Jr., who has furnished the following statement of morbid appearances:—

"Autopsy of Mr. S——, February 13th, 1852. On opening the cavity of the right pleura, a considerable quantity of rather fetid gas escaped, and on raising the sternum the same cavity was found to contain about two pints of a dingy, yellowish-gray purulent fluid. The pleural surface at the anterior part of the chest was covered by a thin coating of recent lymph.

"The right lung reduced to about one-fifth of its natural size, was compressed against the spine and the posterior wall of the chest. Before removing the fluid from the pleural cavity a pipe was inserted into the trachea, and on inflating the lungs the air escaped freely in large bubbles, from a point situated toward the posterior part of the right lung, about the junction of its upper and middle thirds. The right lung, removed from the chest, was found to be completely carnified by pressure, except in those parts occupied by disease. No air cells were visible anywhere, and the whole lung was destitute of crepitation. The lobes were adherent to each other by recent lymph. The

upper third of the lower lobe was occupied by gangrene, which had reached an advanced stage, the pulmonary tissue forming a soft, shreddy, disintegrated mass, of a fetid odor, and a dirty, grayish color and infiltrated with purulent fluid. A gangrenous cavity of considerable size, had apparently existed at this spot before the compression of the lung. The opening by which it had communicated with the pleural cavity was about the size of a goose-quill, and situated posteriorly at the very uttermost portion of the lower lobe. The limits of the gangrenous portion of the lung, were well defined, but only a very little inflammation existed in its neighborhood, the solidification of the pulmonary tissue, except just outside the limits of this gangrenous cavity, being entirely of a passive character, and due to its compression by the pleuritic effusion. The left lung and pleura were healthy. There was no tubercle anywhere. Signed J. C. D.

Remarks.—On comparing the above appearances with the physical forms noted before death, it is evident that the non-vesicular respiration, with a high pitch, which was heard at the summit of the chest posteriorly, was due to the consolidated lung in this situation. The non-vesicular sound, low in pitch, heard in the mammary region, is attributable to the entrance of air into the pleural cavity through the perforation. The low pitched non-vesicular sound heard at the inferior posterior part of the chest was probably owing to the entrance of air into the gangrenous cavity.

FOURTH SERIES.—TUBERCULOSIS.

For convenience of reference, the cases of tuberculosis will be distributed into four subdivisions, according to the amount and stage of the disease, as follows:

1. Small tuberculous deposit.
2. Abundant tuberculous deposit.
3. Tubercle advanced to excavation.
4. Tubercle arrested.

1. *Small Tuberculous Deposit.*

Obs. 1.—January 4. George Laver. Hospital patient. Slight dulness at summit of right chest. Respiration at

summit of right chest relatively feeble, without any aberration of rhythm. The elevation of pitch compared with the left side, is distinct.

Remarks.—The symptoms in the history of this case pointing to the existence of tubercle are as follows:—cough for one and a half years, with slight expectoration. Slight hæmoptysis. Respirations increased in frequency ranging from twenty-four to thirty-six. The patient was not much emaciated nor greatly debilitated; had not been subject to night perspirations or pleuritic pains. Countenance not notably pallid; no febrile movement.

This was one of the earliest observations relative to the subject, when I was satisfied simply to ascertain the pitch of the respiratory sound over the site of the tubercle as determined by percussion, without directing attention to comparative pitch of expiration, and other points. This remark will account for the limited scope of the observations in several other instances.

Obs. 2. —Jan. 4. Slayter. Hospital patient. At the summit of right chest marked relative dulness on percussion. The pitch of respiration is higher than on the left side. Aside from this, a disparity is not apparent, except in the relative feebleness of sound on the right side. No murmur of respiration is appreciable.

Jan. 27th. The following was noted as a distinct observation, without reference to the former: I find relative dulness at the summit of the right chest anteriorly. Over this situation the respiratory sound is relatively feeble. I cannot discover a difference of inspiration between the two sides, nor is there a sound of expiration appreciable on either side. Aside from relative feebleness, the elevation of pitch appears alone to distinguish the respiration on the right from that on the left side.

Feb. 9. The difference in the vesicular quality between the two sides is not marked. The marked difference is in pitch.

Remarks.—The symptoms, in the history of the case, pointing to tubercle, are as follows: cough of four months' continuance, with small expectoration. Night perspirations. Is debilitated and pallid, but not greatly

emaciated. Pulse 88. Respiration 18. Has not had hæmoptysis, nor complained of pleuritic pains.

Obs. 3.—January 28th. Aaron Young. Hospital patient. Slight relative dulness on percussion in right infra-clavicular region. The respiration in the right infra-clavicular region is higher in pitch than in the corresponding region of the left side. The expiration is prolonged, with an interval between the inspiration and expiration. In the left infra-clavicular region the respiration is lower in pitch. There is a short sound of expiration, with a short interval between the two sounds. The inspiration is longer than on the right side. Slight broncophony exists in the right infra-clavicular region.

March 2d. At summit of right chest, the expiration as long as the inspiration. Inspiration and expiration about the same in pitch. Near the sterno-clavicular junction, on both sides, loud bronchial respiration, but much more developed, and higher in pitch on the right side. Expiration more developed on this side, the pitch about the same as that of the inspiration.

March 3d. Expiration and inspiration at summit of right chest apparently about similar in pitch, the expiration notably prolonged. At the summit of the left chest a sound of expiration is appreciable, notably lower in pitch than the inspiration. The contrast between the two sides consists in an elevation in the pitch of both sounds on the right side; a prolonged expiration, about equal in pitch to the inspiration on the right side, while the pitch of the expiration is lower than that of the inspiration on the left side.

Remarks.—I give this among the cases of tuberculosis, but I do not feel positive with respect to the diagnosis.* On another examination made on the day I am writing, viz: March 8th, I find the same physical signs, with marked relative dulness over the right scapula below the spinous ridge. The symptoms in the history which relate to phthisis are cough, since January, with moderate expectoration. No hæmoptysis or pleuritic pains. He entered the hospital for cough and general debility, Janu-

* The correctness of the diagnosis was subsequently demonstrated.

ary 13th. Afterward he contracted typhus, from which he is now convalescent. His cough and expectoration are less at the present time than when he entered. The physical signs at the summit of the right chest may possibly be due to a normal disparity, which, as has been seen, frequently exists to a greater or less extent—in this instance, perhaps unusually marked; or they may be due to uniform enlargement of the bronchi on the right side.

Obs. 4.—Jan 30. Felata Kyser. Hospital patient. Slight relative dulness of resonance in left infra-clavicular region. The dulness is more marked over the left scapula, and in the latter situation there is a distinct broncophony. The pitch of respiration is in a marked degree higher on the left than on the right side, at the summit of the chest anteriorly and posteriorly. The expiration is not prolonged. The elevation of pitch is the chief source of disparity between the two sides.

Remarks.—The symptoms pointing to tuberculous disease are cough, with small expectoration, pleuritic pains; occasional night perspirations; respiration 28; the pulse is 70. She had not had hæmoptysis.

Obs 5.—Feb. 1. Harmon Kramp. Hospital patient. Relative dulness of resonance, and distinct broncophony, in the right infra-clavicular region. The pitch of respiration, in this situation, is notably higher than on the left side. The inspiration is somewhat shortened, the expiration prolonged, and higher in pitch than the inspiration, with an interval between the two sounds.

Obs. 6.—Feb. 2. Mrs. R. Out-patient. On auscultation, before percussing, the respiration is found to be notably higher in pitch in the right, than in the left infra-clavicular region. Aside from this disparity in pitch, the inspiration on the right side is somewhat shortened, with a short sound of expiration; on the left side no sound of expiration is appreciable.

Slight relative dulness of resonance exists in the right infra-clavicular region, and distinct broncophony.

Remarks.—The symptoms in the history of this case pointing to tuberculosis are, cough, troublesome at night,

almost entirely without expectoration; pleuritic pains; considerable emaciation; pallid complexion; night perspirations; tuberculous fever. Respiration 28.

Obs. 7.—Feb. 12. Patient at almshouse; name not noted. Slight dulness on percussion at the summit of chest on the left side. Below, resonance equal on both sides. The respiratory sounds, at the summit of the chest, on both sides, are about equal in intensity. There is an equally short, faint, scarcely appreciable expiration on both sides. The pitch, however, on the left side is notably higher. The vesicular quality is also somewhat diminished on the left side.

Remarks.—The symptoms in this case were not noted. It is simply stated that the history and symptoms point to tuberculosis.

Obs. 8.—Feb. 16. Mrs. R. Private patient. Marked relative dulness on percussion over the right scapula, with broncophony. Slight relative dulness in right infraclavicular region. The respiratory sound at the summit of the chest is notably higher in pitch than on the left side. There is a faint sound of expiration, which is prolonged, and appears to be higher in pitch than the inspiration. On the left side, the sound is lower in pitch, the vesicular quality is more marked, and no sound of expiration is appreciable.

Remarks.—In this case, I was led to the site of dulness on percussion, by the signs developed by auscultation, resorting to the latter method before the former. There are few symptoms denoting, directly, pulmonary disease in the case. The patient has been recently confined, having had three children in the space of five years; lactation protracted during the last pregnancy up to the period of quickening. Since her confinement she has had chills, with irregular paroxysms of febrile movement. She is considerably emaciated and prostrated, presenting a morbid aspect. Cough has been present, only occasionally, and is not a prominent symptom. The expectoration is insignificant; she has not had hæmoptysis, nor distinct pleuritic pains; the secretion of milk was tolerably abun-

dant, but weaning was speedily resorted to, and she has since improved in strength and appearanc, without any further development of pulmonary symptoms. These remarks were written three weeks after the above observations were noted.

Obs. 9.—Feb. 22. Murdoch Gillis. Hospital patient. On immediate auscultation, the pitch of respiration is higher on the right side at the summit. This is more marked on mediate auscultation. The pitch in the right infra-clavicular region is highest toward the acromial angle. There is here a prolonged expiration higher in pitch than the inspiration.

Remarks.—The signs on percussion are inadvertantly not embraced in the observation. It is noted that Mr. Smith, one of the resident students at the hospital, had made and recorded the results of his examination prior to mine, which were afterward compared with the above and found to correspond.

The symptoms in the history pointing to tubercle, are as follows: cough of eleven months' standing; moderate expectoration; considerable emaciation, pallor and debility, and, of late, slight perspirations at night. Has not had hæmoptysis nor pleuritic pains. Pulse 108; respiration 30.

Obs. 10.—Feb. 24. Robert Evans. Out-patient. Slight dulness on percussion in right infra-clavicular region. The pitch of respiration in this region is higher than on the left side. There is no appreciable sound of expiration in this situation.

Posteriorly, over the scapula above and below the spinous ridge, there is marked relative dulness on percussion. The respiratory sound in these regions is very feeble; but, in forced respiration, the pitch is determined to be notably higher than on the left side, and a sound of expiration is heard as high or higher than the sound of inspiration. Moderate but distinct broncophony exists at the summit of the right chest, anteriorly and posteriorly, and not on the left side.

Remarks.—The patient has had a cough for nine months,

with small expectoration. Has lost considerable flesh and strength, and is incapacitated for labor, but is able to be up, and to walk out of doors. Respiration 24. Other details pertaining to the history were not recorded.

Obs. 11.—March 3. Louisa Stately, aged 8 years. Hospital patient. Distinct dulness on percussion in left infra-clavicular and over the scapular regions. The respiratory sound is more developed in the left than in the right infra-clavicular region. A difference in duration of the inspiration, or in the vesicular quality is not appreciable. There is a feeble sound of expiration on both sides. The chief point of disparity pertains to the pitch, and this is marked. It is higher on the left side. The expiration on this side appears to be about equal in pitch to the inspiration, while it is lower than the inspiration on the right side. The pitch is also notably higher on the left than on the right side, over the scapular region.

Remarks.—This patient has had cough for several months, and, once, slight hæmoptysis. She is thin and pallid. Before my examination in this case, Mr. Bowen, medical student, had made an examination with reference to the pitch of sounds, and recorded the results, which were found afterward to correspond with those obtained by me, save so far as the latter relate to the expiration, of which he made no note.

2. *Abundant Tuberculous Deposit.*

Obs. 1.—Jan. 4. Biselay. Hospital patient. Marked relative dulness of resonance at the summit of the left chest posteriorly, and of the right chest anteriorly. Respiration, on both sides, tubular, with prolonged expiration.

Distinct relative elevation of pitch at the summit of the left chest posteriorly, and of the right chest anteriorly.

Remarks.—In the above, as in other observations made shortly after my attention had begun to be directed to the study of the pitch of respiration, the fact of the disparity of pitch alone was noted, without reference to the expiration and other points.

Obs. 2.—Jan. 4. Julia Moniva. Hospital patient. At summit of right chest anteriorly, marked relative dulness on percussion. On auscultation, sonorous râles occasionally on both sides, at the summit of the chest. Respiratory sound more developed on the right side. No expiratory sound on either side. On the right side, distinct elevation of pitch. Distinct broncophony on the right side.

On another examination after the foregoing was noted, the patient in the mean time having coughed and expectorated, I find the respiratory sound more developed at the summit of the left chest, and on comparing the pitch, the elevation is more strongly marked on the right side than before. The disparity in pitch is the most strikiug point of difference between the two sides in this case.

Obs. 3.—Jan. 5. Welch. Hospital patient. Marked relative dulness on percussion at the summit of the left chest. Respiratory sound feeble, and tubular on the left side, presenting a marked elevation of pitch compared with the respiration on the right side.

Obs. 4.—Feb. 19. Daniel Cuit. Hospital patient. On immediate auscultation, prior to examination by percussion or inspection, the respiration at the summit of the right chest is found to be notably higher in pitch in comparison with that at the summit of the left chest. The inspiration on the right side is shorter than on the left. On the right side there is a feeble, prolonged sound of expiration, with an interval between the two sounds. The expiratory sound is too feeble to determine the pitch. On the left side there is a feeble, short sound of expiration, which is continuous with the sound of inspiration.

Afterward, it was found that the summit of the right chest is notably dull on percussion, and marked broncophony exists on this side.

Obs. 5.—Feb. 20. Evelina Potter. Hospital patient. On examination by auscultation, before practicing percussion or inspection, with a view to determine on which side tuberculous deposit might be present, or more abundant, I found, at the summit of the right chest, the inspiratory sound higher in pitch than on the left side; the vesicular quality less; a sound of expiration higher in pitch than the sound of inspiration, prolonged to the same length as

the inspiration, and an interval between the two sounds.

At the summit of the left chest the sound of inspiration is feeble, but lower in pitch than on the right side. A prolonged expiration exists on this side, high in pitch, with an interval between the two sounds.

On percussion there is marked relative dulness on the right side over the infra-clavicular and scapular regions.

An occasional sibilant rale is heard in the right infra-clavicular region.

Broncophony exists on both sides, but is much more marked on the right side.

3. *Tubercle Advanced to Excavation.*

Obs. 1. *With Autopsy.*—Jan. 25. Hugh McMullin. Hospital patient. Marked relative dulness on percussion, and broncophony at the summit of the right chest. Respiration over right chest at summit presents a notable elevation of pitch on comparison with the summit of the left chest. The expiration on the right side is prolonged, exceeding in duration the sound of inspiration. The inspiratory sound is shorter than on the left side and there is a longer interval between the two sounds, but less in duration than on the right side. On the left side the sound of expiration is higher in pitch than that of inspiration.

Jan. 31. Over a space in the right infra-clavicular region, about midway between the acromial and sternal sides of this region, the respiratory sound is lower in pitch than in every direction surronnding this space. A feeble sound of expiration is heard over this space. No pectoriloquy. Above this space, directly upon the clavicle, the respiration is intensely tubular, with prolonged expiration, the pitch quite high, resembling tracheal respiration.

In the left infra-clavicular region, over the space corresponding to that just referred to, (*i. e.* just below the clavicle, about midway between the two extremities,) the respiratory sound has a high pitch, the expiration being prolonged.

The right side presents, on percussion, a higher pitch of resonance, but a hollow, or tympanitic sound.

On immediate auscultation over the summit of the chest the right side gives the lower pitch. It is to be observed

in this case, that the side on which the percussion pitch is highest, has the lower pitch of respiration.

Feb. 1. A *bruit de pot fêlé* is to-day distinctly apparent over the space in the right infra-clavicular region, characterized by the gravity of the pitch of respiration. The latter fact is also again verified.

Feb. 18. Confirmed again the statements made in the foregoing observation, and observed, in addition, that over the space in the right infra clavicular region presenting a low pitched sound of respiration, the expiration is lower in pitch than the inspiration. On examination of the left infra-clavicular region yesterday, and to-day, I find a space about midway between the acromial and sternal extremities, and about half an inch below the clavicle, in which the sound is blowing, *i. e.* non-vesicular, and lower in pitch than in every direction surrounding it. There is a sound of expiration here which is lower in pitch than the sound of inspiration.

This case proving fatal on the 21st of Feb., the lungs were examined on the 23d, by Dr. John C. Dalton, Jr., who has furnished the following statement of morbid appearances:—

"*Right lung.*—The upper, and one-half of the middle lobe are entirely without crepitation, being completely solidified except at situation of cavities, by deposits of yellowish, cheesy tubercle, and gray infiltration. The greater part of the upper lobe is occupied by a superficial irregular cavity nearly empty, twice or more as large as a hen's egg, situated quite at the apex, and rather toward the outer part of the lobe. No other cavity of any considerable size in the right lung. The lower lobe crepitates pretty freely, but contains also many small yellowish tubercles. Many miliary tubercles sprinkled over the pleural surface of the lower lobe.

"*Left lung.*—Apex of upper lobe crepitates freely, and is nearly healthy, but its middle portion is extensively solidfied by yellowish tubercle, and by well-defined spots of light-red hepatization. About the middle of the anterior portion there is a superficial cavity about the size of a walnut, containing a drachm or so of pure pus. Remainder of the left lung somewhat tuberculous, but less so than other parts."

Remarks.—It is evident, on comparing the foregoing

morbid appearances with the preceding observations, that the low pitched respiration (consisting of a low inspiration and a lower expiration,) heard over a circumscribed space in either infra-clavicular region, was due to the cavities existing in the lungs on both sides, while the high pitched respiration, heard in the neighborhood of these spaces in every direction, was owing to the solidification of the lung surrounding the excavations. The latter, in other words, was the bronchial, and the former the cavernous respiration. It may seem, at the first glance, that the results of the first of the several examinations conflict somewhat with those of the subsequent examinations. The apparent inconsistency may be readily explained. Auscultation with the stethoscope in certain portions of the right infra-clavicular region, that is, over the portions of lung solidified, would, of course, give a high pitch of sound. That the low pitched respiration, denoting excavations, was not discovered at the first exploration, may have been because the whole of the infra-clavicular region was not carefully examined, or the cavities may have been filled with fluid secretions, and therefore the conditions for the cavernous respiration were not present.

Obs. 2.—Jan. 26. James Bell. Hospital patient. On the left side, marked relative dulness on percussion at the summit.

Over the right side, at the summit, the respiration is evidently supplementary, the murmur being intense, but vesicular, without any sound of expiration. The resonance on percussion on this side is quite clear. The pitch of respiration, however, is rather high.

Over the left chest, at the summit, the respiratory sound is lower in pitch than over the right side. The pitch, however, is not uniform over the whole of the summit of the left side. Toward the acromial angle of the infra-clavicular region, over a space of about the dimensions of the thoracic extremity of the stethocope, the pitch is low. At a short distance, in a direction toward the sternum, the pitch is notably higher. Over the latter space, compared with the former, the sound on percussion is in a marked degree dull. The resonance over the former space seems to me somewhat tympantic in character, and the tone is

communicated through the stethoscope with a resonance approaching to pectoriloquy.

On applying the ear immediately over the summit of the left chest, the pitch is lower than on the right side.

The history of this case shows copious muco-purulent expectoration.

Obs. 3. *With Autopsy.*—Feb. 2. O'Donnell. Hospital patient. On the right side the resonance is relatively dull in a marked degree. On immediate auscultation, the pitch of respiration is found to be notably elevated, with a prolonged expiration, the latter being quite high in pitch. About an inch and a half, on the right side, below the clavicle, toward the acromial angle the pitch is low, devoid of vesicular quality. At the same distance below the clavicle, toward the sternal extremity the pitch is high.

Feb. 13. On examination of this patient to-day, I observed at the summit of the right chest, during the first part of the act of respiration, a tubular respiration loud and high in pitch; and during the latter part of the inspiration, the inspiration abruptly became low, the blowing character being still preserved. The contrast between the two portions of the inspiration was striking. There is a feeble sound of expiration which is low in pitch.

This case terminated fatally a few days after, and the following statement of the morbid appearances of the lung is furnished by Dr. John C. Dalton, Jr.:—

"The right lung is closely confined by old and very firm adhesions throughout its upper portion. Whole of upper and most of middle lobe destitute of crepitation, and solidified by gray (tubercular) infiltration. Lower lobe full of air, but shows also many small, well-defined cheesy and softened yellowish tubercles. Upper lobe toward median line, solid, or with only one or two small cavities. Toward sternal portion there is a large superfical cavity, with irregular walls, capable of holding an egg or more, containing a few drachms of pure pus, and lined throughout by a grayish, semi-transparent false membrane, about a line and a half in thickness; easily scraped off by the edge of the scalpel. This cavity communicates with other smaller ones at posterior part of upper lobe, and also with a branch of the right bronchus about the size of a goose-quill.

"The left lung shows many small, yellowish, cheesy, and

softened tubercles, and one cavity at its posterior part nearly or quite empty, and exhibiting the appearance of having been for some time inactive. But very little gray infiltration in the left lung, and it is altogether much less diseased than the right. No effusion in either pleural cavity. (Signed) J. C. D."

Remarks.—The low pitched sound of respiration, with the expiratory sound lower than the inspiration heard over a circumscribed space in the right infra-clavicular region, was doubtless due to the cavity discovered in the lung on this side at the autopsy. The high pitched sound in a direction from the cavity toward the sternum was produced by the solidified lung. The latter sound, in other words, was bronchial, and the former cavernous. The combination of the bronchial and cavernous respiration in the act of inspiration is an interesting point in this case, the former due to the current of air in the bronchial tube passing through solidfied lung to communicate with the excavation, and the latter produced by the entrance of air within the cavity.

The examinations were limited to the anterior part of the chest, owing to the great feebleness of the patient A careful exploration over the posterior summit of the chest, on the left side, might have led to the pitch variations incident to the cavity in the posterior part of the left lung.

Obs. 4—Feb. 22. John Jervis. Hospital patient. On immediate auscultation, the pitch of respiration is found to be higher in the right infra-clavicular region. The respiration is short and non-vesicular. On immediate auscultation, over a space near the sterno-clavicular junction, the pitch is low, and the sound non vesicular. A sound of expiration is not sufficiently appreciable to determine the pitch. The sound over the space just referred to contrasts strongly with the high pitched bronchial respiration over the sterno-clavicular junction. The chest is depressed on the right side in the infra- and post-clavicular regions. It expands less in these situations, on inspiration, than on the left side.

On percussion, the right infra-clavicular region is found to be relatively dull. Over the space referred to, the

sound is lower in pitch than over surrounding portions, and somewhat tympanitic. Over this space there is forcible vocal resonance, but not marked pectoriloquy.

Over the right mammary region there exists marked relative dulness on percussion, almost amounting to flatness. There is not much resonance, but comparatively a greater degree on the left side. The respiration on both sides over this region is feeble, but on the right side notably higher in pitch.

Remarks.—The disease in this case had existed for about six months. The respirations were 32, and the pulse 128. The comparative lowness of the pitch of respiration, with the absence of the vesicular quality, over a space in the right infra-clavicular region, is presumed to denote the existence of a cavity in that situation. In other words, the respiration in this situation is cavernous.

In this case, at my request, Mr. Bowen, medical student, examined and recorded the results, which were found to correspond with mine.

Obs. 5.—March 5. Carlos Aberd. Hospital patient. This patient entered in the last stage of the disease, and was so greatly prostrated that the physical examination was very cursory. Death occurred the second or third day after his admission, and an autopsy was not practicable. It is simply noted, that on the right side, at the summit of the chest, before and behind the respiration is non-vesicular, low in pitch, with an expiration lower in pitch than the inspiration.

Obs. 6.—Feb. 26. James Kelly. Hospital patient. The examination for pitch is difficult on the left side, owing to the presence of râles. There exists marked relative dulness on percussion over the left side, at the summit of the chest anteriorly and posteriorly. On the right side, posteriorly, the respiration over the scapula is high in pitch, with a prolonged expiration higher in pitch than the inspiration.

On the left side (the side in which relative dulness exists) over the scapula the sound of respiration is lower in pitch, and devoid of the vesicular quality. An expiration is present lower in pitch than the inspiration.

In the right infra-clavicular region toward the sternal

boundary, the pitch is high, and the sound loud, without an appreciable expiration. Toward the acromial border, in the same region, the pitch is low, with an expiration lower than the inspiration. The sound here, however, has the vesicular quality. Percussion over the half of this region in the direction of the sternum is dull, while over the other half it is clear.

In the left infra-clavicular region, râles obscure the pitch save over a space near the sternum. Here the pitch is low, with an expiration lower than the inspiration.

Obs. 7, *with Autopsy.*—Jan 27. This case is included among the cases of tuberculosis advanced to the stage of excavation. It was not, however, really a case of that description. The existence of a cavity was predicated upon certain pitch characters, and other signs, but the autopsy proved the diagnosis to be incorrect. Candor requires the observation to be given in connection with the post-mortem appearances. The case is interesting and instructive, showing circumstances which were perhaps well culculated to lead to error of diagnosis. It also illustrates, incidentally, certain points connected with the present subject.

James Caytor, aged 5 years. Hospital patient. On percussion, the resonance in the left infra clavicular region appears to be somewhat tympanitic, but the pitch of resonance is distinctly higher than on the right side. There is a distinct *bruit de pot fêlé* in the left infra-clavicular region. The pitch of respiration is distinctly lower in the left infra-clavicular region than in the right. There is a feeble expiratory sound on this side, not uniformly, but occasionally appreciable. The pitch is lower in the above situation than on the same side over the mammary region.

Over the right infra-clavicular region, the pitch of respiration is higher than over the left mammary region. Pectoriloquy is not present. In the left infra clavicular region the second rib yields with very little resistance to pressure, in this respect contrasting strongly with the resistance over the corresponding region on the right side.

Jan. 29. Confirmed the foregoing facts. The *bruit de pot fêlé* is distinct.

The case terminated fatally early in the present month (March,) and the following statement of the morbid ap-

pearances of the lung is furnished by Dr. John C Dalton, Jr.:

"The pleuræ of both lungs were rather plentifully sprinkled with tubercular granulations—no effusion or adhesion. There were also a few small tuberculous masses in substance of middle and lower lobe of right lung, yellowish and firm in consistence; no cavities and no softened tubercles. The largest collection of tubercle was just behind the root of the left bronchus, firm and cheesy—about the size of a small English walnut. Bronchial glands slightly tuberculous. Bronchi of natural relative size and length. There was considerable redness of mucous membrane of the left bronchus, with three or four shallow white ulcerations. No tuberculous deposit perceptible at base or in neighborhood of these ulcerations. No similar affection of right bronchus. Apices of lungs nearly or quite free from tubercle. No pneumonia anywhere. (Signed) J. C. D."

Remarks.—A cavity was sought for in the left infraclavicular region, supposed to be due to excavated tubercle or pouch dilatation of the bronchus in this situation. This expectation was based on the physical signs above mentioned, viz: the low pitch of respiration, the tympanitic resonance on percussion, and the *bruit de pot fêlé.* The diagnosis was formed with considerable hesitation, because the rational system did not denote extensive tuberculosis advanced to the stage of excavation. The respirations were not accelerated nor labored. There did not appear to be much expectoration. The age of the patient militated against the supposition. The patient was reduced to marasmus, and died by asthenia, evidently from defective assimilation. In spite of these circumstances, the pitch characters, and the cracked-pot, tympanitic resonance, were thought to denote the existence of a cavity.

The source of the error is intelligible. The thoracic walls were very thin, and the respiratory sound was largely developed. The respiration on the right side at the summit, was higher in pitch from the predominance of the bronchial element as a natural disparity. I was not, at that time, so well prepared to attribute a disparity so strongly marked to a normal difference, as since the ex-

amination of a number of healthy chests with reference to this among other points. The relative lowness of the pitch on the left side did not arise from a depression on this side, but because the normal elevation of pitch on the right side was in this instance strikingly marked. Instead of a cavity, there existed in the right lung a solid tuberculous deposit of about the size of an English walnut. This was situated directly behind the left bronchus. In this situation it would not evidently be expected to raise the pitch of the bronchial respiration in front on this side, and by making some pressure on the tube it would be likely to render the sound less developed.

How is the *bruit de pot fêlé* to be accounted for? It is well known that this modification of tympanitic resonance does not universally require a cavity for its production. It has been observed in the second stage of pneumonitis. Without discussing the mode of its production as a rule in cases in which cavity does not exist, I will offer an explanation of its occurrence in this particular case. It will be seen by reference to the observation, that the second rib on the left side was remarkably yielding to pressure. The solid tuberculous mass behind the bronchus furnished a point of resistance, or, as it were, a fulcrum. On percussion, the ribs and the lung anteriorly to the bronchus yield readily, and the tuberculous deposit formed a kind of anvil causing the air in the bronchi to be expelled in such a way, and with sufficient force to yield the cracked pot sound. Of the existence of this sound in the case there was no doubt. While the patient was in the hospital, it was frequently demonstrated by myself and others as a good illustration of the sign.*

I can only account for the fact that the pitch at the summit of the left chest appeared lower than over the mammary region by supposing that the presence of the small tuberculous masses in the lower lobe on this side, exalted the pitch more than the deposits in the upper lobe. Why this should be so it is not easy to understand. It is possible that the comparison, in these situations, was not made with sufficient care.

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* Since this was written, a child admitted into the hospital, with affection of the mesenteric glands, reduced to extreme emaciation, but without cough or other symptoms of pulmonary affection, presents a very distinct *bruit de pot fele* on the left side.

This case furnished an interesting illustration of the effect of a moderate tuberculous mass, surrounded by vesicles, in modifying the pitch of resonance on percussion. The pitch of sound on percussion, in the left infra-clavicular region, was distinctly raised.

4 *Arrested Tubercle.*

Obs. 1.—Jan. 27. Martin Welch. Hospital patient. This patient presented, a year ago, the evidence, physical and rational, of small tuberculous deposit. The disease, in the mean time, has apparently made no progress. He remains in the hospital as a porter. Some cough and moderate expectoration continue. He has not lost in weight during the year, and his aspect is not notably morbid.

On auscultation, the respiration at the summit of the right side is relatively feeble, and the pitch is elevated. There is no sound of expiration, nor does the respiration present any abnormal characters, save feebleness and elevation of pitch.

On percussion, after obtaining the foregoing results, I find slight relative dulness in the right infra-clavicular region.

Obs. 2—March 4. The subject of this observation is a professional friend of the writer. In 1844 he suffered for several consecutive months from cough with moderate expectoration. Hæmoptysis occurred during this period once. His weight was considerably diminished. He gradually recovered without medical treatment, and with the exception of a cough, which lasted for two months in 1849, he has enjoyed good health, being entirely free from any symptoms of pulmonary disease.

There is distinct elevation of the pitch of resonance on percussion at the summit of the chest on the left side. The respiration in this situation is slightly elevated in pitch, and the vesicular quality is somewhat less developed than on the right side.

Slight vocal resonance exists on both sides, in about an equal degree.

Remarks.—The variations in resonance and respiration being on the left side, are, on this account, the more significant of changes, probably due to arrested tubercle.

Practice in Philadelphia.—From the Philadelphia Medical and surgical Journal, the following extract is taken:

"Two-thirds of the practice of medicine in Philadelphia, even among those who pay for everything else, is done gratis; and not unfrequently insult is added to injury. This is due to the crowded state of the profession, and to the *mock charity* inculcated by many of the public teachers. The average fee for medical attendance, *paid*, does not exceed *twenty five cents* a visit."

Perhaps some one, more familiar with the phases of professional income in Boston than ourselves, may favor us with a statement of the profits of practice here. Some certainly have large incomes; while others, whose merits and qualifications cannot be questioned, have none at all. *Boston Med. Jour.*

Friendly Injury.—In the Bee, of this city, the following statement appeared the past week:—"A suit was lately brought in Barnstable county, which grew out of the simple act of shaking hands. The defendant, it appears, seized the hand of the plaintiff to shake it, and in doing so, he grasped it so tightly as to crush the bones and thereby cripple it forever. The hand became ulcerated, and many of the bones have been discharged from the wound. The result of the trial is not yet heard."—*Ibid.*

Mineral Springs.—Dr. John Bell, [Philadelphia,] who is preparing a work on mineral springs, more especially on those of the United States, is desirous of procuring, at an early day, all accessible information on the subject. With this view he requests his professional brethren to transmit to him all the facts in their possession which may throw light on the chemical composition and curative powers of the waters of the springs in their respective neighborhoods.

Proprietors of these waters would oblige by sending to Dr. Bell authenticated accounts on these points, and also of the topography of the springs, and the roads by which they are approached.—*Ib.*

On the use of Manganese as an Adjuvant to Iron.—M. Pétrequin quotes various authors to prove that manganese is a normal constituent of animal and vegetable tissues; and believes that wherever iron is present in appreciable quantity, manganese coexists with it. Hence iron alone will not always succeed in blood diseases. M. Pétrequin has observed many cases of chlorosis, which have resisted iron as obstinately as anæmia connected with cancer or organic degeneration. Other cases again, after deriving a certain amount of benefit from iron, remain stationary. Others again appear cured by iron, but the cure is not permanent. The remedy required in these cases, M. Pétrequin finds to be manganese. He does not give it or iron alone; but combines them.

It is espesially in *diseases of the blood* that ferro-manganic medicines are useful. They have a special action on the vascular apparatus, on the formation of the blood, and on the circulating fluid itself. They do not act merely as tonics or astringents; but are regenerators of the blood. They have succeeded admirably in anæmia following hemorrhage, operations, polypi, metrorrhagia, &c., also in the chlorosis attending puberty, which is a more common disease than is generally supposed, and occurs even in males. M. Pétrequin has also frequently found the combination of iron with manganese of benefit in the diseases of women at the critical period. He has often seen in these subjects, *metrorrhagia*, accompanied with an aspect of the surface which would lead to the suspicion of organic uterine disease: the hemorrhage, however, was but a complication, and the patients, apparently in a hopeless state, have recovered under the use of ferro manganic preparations, conjoined with tonics and ergotine.

In *amenorrhœa* and *dysmenorrhœa*, the patients often imagine that they require to be bled; but care must generally be taken not to comply with this request. M. Pétrequin has more than once seen cases of amenorrhœa with severe chlorosis, in which it has not been desirable to hasten the appearance of the catemenia—the consequent loss of blood aggravating the disease. The general state of the health must here be carefully attended to. Œdema of the lower limbs sometimes occurs in these cases; but it is a less severe complication than when it attends metror-

rhagia. It often disappears, as the patient recovers, under the use of iron and manganese.

These medicines are no less efficacious in the treatment of *anæmia* resulting from prolonged intermittent fevers, prolonged suppuration, strumus, syphilitic, or cancerous affections, phthisis, &c. Pills and the syrup of iodide of manganese and iron are preferable in these cases.

In all these cases, the ferro manganic preparations do not merely act on the stomach and the nervous system, but they are absorbed and assist in the formation of hæmatosine and new blood globules, so as to restore the blood to its normal condition. Their effect in this way is greater than that of iron alone.

In the *functional affections of the heart* connected with chlorosis and anæmia, and which must not be mistaken for organic disease, a combination of iron and manganese with digitalis and other moderators of the heart's action is advantageous. The same remark applies to the *functional disorders of the lungs*, attending the same constitutional states. *Disordered states of the nervous system* are intimately connected with those of the blood. M. Pétrequin has found that the ferro manganic preparations succeed well in these, even though uncomplicated with chlorosis. He, as well as M. Gubian, has observed that iron is here better tolerated when combined with manganese. He has also seen benefit from the use of iron with manganese in many cases of *dyspepsia, gastralgia, gastro-enteralgia.* Nervous affections of the digestive organs are often the result of chlorosis; and where stomachics and cinchona have failed, iron has often been found (especially the carbonate, by some English physicians) to be of service. Gastrodynia complicating chlorosis has often yielded to the use of ferro-manganiferous water; and to pills of carbonate of iron and manganese.

In *nervous affections connected with exhaustion* from venereal excesses, onanism, rapid growth, &c., as well as in leucorrhea, diabetes, &c., M. Pétrequin has a high opinion of these medicines. He is continuing his researches on their action in certain cases of sterility from asthenia and in some hypostheric affections of the scalp, such as early baldness, alopecia, &c.

M. Petrequin has confined his observations to a limited number of the ferro-manganic prepartions; and has made many observations before publishing the formulæ which

he finds most useful. Having found even at an early period, that the medicines were liable to adulteration, he has availed himself of the assistance of competent pharmaceutists. Since the publication of his first memoir, in 1849, these medicines have been extensively used in the south of France and in foreign countries.

The formulæ are few and correspond to the preparations of iron generally used in France. They are; 1. *Pills* of carbonate of iron and manganese, or of iodide; 2. *Lozenges* of lactate of iron and manganese; 3. *Syrups* of lactate or of iodide of iron and manganese; 4. Ferro-manganic *Chocolate;* 5. *Effervescing solution* of iron and manganese.

It has been observed that manganese not only preserves water, but purifies that which has undergone change—(Martin-Lauzer.) Ferro-manganic waters (of which there are many in France and other parts of the continent) can be preserved and carried to a distance;—which cannot generally be done with simple ferruginous waters.

M. Pétrequin commences by giving a powder of iron and manganese, with some vinous drink; he then administers two pills daily, one before breakfast and one before dinner, replacing them soon by the lozenges. The syrups and chocolate complete the treatment. He gives the medicines at meal time. The syrup he gives before breakfast, in doses of a tea-spoonful; and he finds it useful to administer directly after it some infusion of centaury, or of chamomile flowers and orange.

Large doses are unnecessary and useless; for they are liable to produce irritation of the stomach and exhaustion of the nervous system; and the reparation of the blood is slow and progressive, and cannot, even were it desirable, be effected rapidly. Besides the iron and the manganese are not absorbed in any greater quantity, if large doses are given.—*Dublin Med. Press.*

On the Internal Administration of Chloroform in Delirium Tremens. By Richard G. H. Butcher, F. R. C. S. I., Surgeon to Mercer's Hospital, &c.—The patient, Wm. Magrath, aged 26, a powerful young man, by trade, a wine porter, was admitted by the author, into Mercer's Hospital, June 25th, 1852. When admitted, four days

had elapsed from the time of his giving up the stimulus, and he had no sleep during that period. His countenance was particularly anxious, with a wild expression: the pathognomonic symptom, tremor of the tongue and hands fully established. His speech was hurried and uneven; he was quite irrational and wild; pulse 120; surface of the body hot and burning, while his face was covered with perspiration, and his hair drenched in sweat. He was put into bed but would nat remain quiet; got up and kept constantly walking up and down the ward and corridor. He was ordered two grains of calomel and a grain of opium in pill, to be taken every third hour. He had taken three, but each was vomited almost immediately after being swallowed. A draught containing one grain of morphine, two drops of creosote, and an ounce of camphor mixture, to be given every third hour, was next tried, but this was likewise rejected. If the patient only took a sup of cold water to moisten his parched mouth and lips, it was instantly vomited.

On the following morning, the 20th, his condition was a great deal worse, and the case now assumed a serious aspect. From the irritability of the stomach, opium in any form could not be got to rest upon it. As for the idea of administering repeated small opiate enemata in this powerful, restless, and uncontrollable young man, the practicability of it could not be entertained for a moment. From the satisfactory issue of two cases, reported in the *American Journal of Medical Science* for January, 1852, the same practice was determined on—the internal administration of chloroform.

At ten o'clock this morning, (26th,) one drachm of pure chloroform in two ounces and a half of water was administered. In an hour after swallowing it, the patient became comparatively tranquil, and could be persuaded to lie in bed.

Eleven o'clock:—He began to get drowsey, and slept for periods of from ten to twelve minutes at a time. At a quarter before one o'clock, he became fully affected by the medicine, and fell into a quiet steady sleep; and on visiting him at two and four p. m. he was still in a profound sleep, and continued so until seven in the evening. During this long sleep of six hours, he was calm and quiet; his pulse fell from 120, which it was in the morn-

ing, to 96, at which it remained; his respirations were between 16 and twenty in the minute, and not louder than natural; the temperature of the body was exalted. All along heat was maintained to the feet, and a pure current of air circulating around him, the windows being kept open. On his awakening, he was nearly or quite sensible, and advantage was taken of this pause to administer a full stimulant cathartic, consisting of six grains of calomel and ten grains of camphor, not only with the intention of freeing the bowels of accumulated matter, but likewise to guard against congestion of the brain. Orders were left in case he should not sleep before ten, to administer half a drachm of chloroform in two ounces of camphor mixture.

27th, ten A. M.—The patient went to sleep almost immediately after swallowing the bolus on last evening, so he did not require the chloroform draught. His bowels were opened three times very freely during the night, and his condition is in every way greatly improved. He is quite rational, and answers every question sensibly; his pulse 96, considerable volume; skin cool; after being interrogated, he quietly turned on his side and went to sleep.

Three P. M.—His bowels have been several times opened since morning, yet his pulse has risen to 110; the temperature of his body is also increased; he is hot and burning; altogether he is excited, and the fear of horrible objects around him has returned. On the presence of those symptoms the chloroform draught was at once repeated. Shortly after, he took a large drink of tea, which was inadvertantly left beside his bed, which produced vomiting immediately.

Nine P. M.—Since the last visit, the patient has slept at short intervals, for one and two hours at a time; pulse still up to 110. Ordered the chloroform draught, one drachm to two ounces and a half of the camphor mixture, to be repeated.

28th:—After the patient had taken the draught last night, he fell into a quiet sleep, which continued uninterrupted until eight o'clock this morning. He awoke quite collected and rational; his pulse 80; skin cool; his tongue and extremities quite free from tremor, and he feels in every respect well; his appetite has returned, and all food

is retained on the stomach. Ordered a grain of morphia in an ounce of camphor mixture, to be given at night.

29th:—This morning the patient is quite restored; he is sitting up eating his breakfast heartily in bed; in short, he is quite convalescent, and only requires a little nourishment to remove the debility consequent upon so severe a struggle.

In reference to the administration of chloroform in the foregoing case remarks the author in conclusion, there is one point which solicits our closest attention, namely: the remarkable lowering of the pulse, when the perfect effect of the medicine was produced; the pulse, in fact, might form the index to direct the practitioner as to the propriety of a repetition of the dose. Again as a precautionary measure, I consider it desirable to keep the heat to the feet, and a current of pure air circulating around the bed and through the apartment in which the patient lies.—*Dublin Medical Press.*

Diarrhœa Successfully Treated by Sulphuric Acid.—Although Sulphuric Acid was recommended in passive diarrhœa, by the late Anthony Todd Thompson, in the Edition of his Dispensatory, published in 1837. I believe attention was only directed to it some twelve or fifteen months ago, by a writer in one of the Medical Periodicals; and according to received notions, no remedy would be less likely to check an attack of this disease, for the state of the tongue, the sour taste in the mouth, and the acid matters rejected from the stomach, would seem to contraindicate its use, and few practitioners venture to have recourse to any other remedies than the old ones of calomel and opium, or astringents and absorbents; which correct the acidity of the primæ viæ and improve the character of the secretions. I am induced to relate a few cases cured by this remedy, as I believe it will be found to be very efficacious in the treatment of this often troublesome disease.

Case 1st. James McCrae, aged 42, labourer, after exposure to wet, was attacked during the night of 22d June, with diarrhœa, succeeded after a time with severe griping pains in his bowels, accompanied with vomiting. Early

in the morning, he procured from a neighboring apothecary shop, an ounce of castor oil, with 30 drops of laudanum, which he retained upwards of half an hour, when it was vomited up with a large quantity of very acid liquid, from which he felt much relieved for a few hours; when towards the middle of the day, all the symptoms having returned, with the same violence as in the morning, he came under my care, and I resolved to try the effect of Sulphuric Acid. I therefore, ordered a mixture containing ℥ss. of diluted Sulphuric Acid, with ℥viiss of water. ℥l to be taken every two hours. The first dose was vomited up within five minutes, but on taking the second which he did immediately on the rejection of the first, he had no return either of vomiting or griping pains, and the next dose checked the purging; he had no recurrence of it from that time, and the following day returned to his work as well as ever.

Case 2nd. Occurred a few days after the above, and was that of a lady who had just recovered from her accouchment, when she was attacked with nearly the same symptoms as the previous case; and was, when I first saw her, much exhausted from the enormous evacuations. I immediately ordered the same medicine, and two doses checked all the symptoms, although she took a third dose, as a matter of precaution, and now speaks to all her friends, of the acid mixture, as a specific in diarrhœa.

Cases 3d and 4th. Were in children—one two years old, and the other five; both were cured in two days, with the Sulphuric Acid, of course in much smaller doses, and sweetened at the time of administering it. I shall not detail many other cases, that have fallen under my care since these, but they have been of sufficient number to give me full opportunities of testing the value of sulphuric Acid in diarrhœa, and from the experience I have had in its use, I can confidently speak of its curative power in such cases.—*Canada Med. Jour.*

On the Proportion of the Subjects bitten by Mad Animals, who become affected with Hydrophobia. By Professor Renault.—M. Renault, Clinical Professor at the Veterinary School at Alfort, has recently presented to the Acade-

my a valuable Report, adverse to the claim again recently set up for the prophylactic power of mercurial inunction in hydrophobia. As this claim has been over and over again refuted, we should not have adverted to the subject, but for the valuable incidental matter introduced by the Professor into his Report. He observes, that before we can receive the numerous cases which have been published of the preservative power of mercurial salivation, it is necessary,—1. That it should be placed beyond all doubt that the animal inflicting the bite was really mad; and that all the persons supposed to have been preserved have really been exposed to the infection. On examining the histories of these cases, however, such proof is quite defective, as in some of those related—e.g., by Ehrmann and Audry, the persons had not been bitten at all, but had merely lain or drank with those who had, or had wiped away their saliva.—2. Next, when persons have been really bitten, and the animal has been really mad, we must be satisfied that the poison has been deposited in the wound. But numbers of the persons referred to have been bitten through their clothes, and every one knows how small a proportion of such acquire hydrophobia, even when no treatment at all has been employed.—3. But even when the poison has been duly deposited in the wound, to admit the preservative power of the mercury, we must allow that all such persons would necessarily suffer from hydrophobia if left to themselves. Numbers of cases contradictory of this are, however, on record. But although all careful observers admit this, the *proportion* of such as escape has never been duly investigated; and the subject being one of the highest interest, M. Renault takes the opportunity of communicating the results of the investigations he has been conducting upon it at Alfort since 1828.

To prevent confusion, he arranges the cases he has collected in two categories. (A.) Cases of dogs or other animals accidentally bitten by others, either mad or supposed to be so, and sent by the police to Alfort, to remain under inspection. Of 224 dogs so brought, between 1827 and 1837, which continued under observation for more than two months, without undergoing treatment, 74 or nearly one-third became mad, and 130 or two-thirds exhibited no symptoms. It is evident, however, that these

cases do not afford any measure of the activity of the virus; for the dog that caused the bite may not always have been mad, the bites could not always be verified, and the hair of the animal may have prevented the poison from penetrating.

(B.) For this reason, another series of facts is adduced. From 1830 to the present time, certain dogs known as really mad have been made to bite, at the Clinical School, other dogs or herbivora in portions of the surface of delicate structure and devoid of hair, or inoculation has been performed with some of their saliva, collected during the height of the paroxysm. Of 99 dogs, horses, and sheep, so treated, 67 became mad, and 32 continued under observation more than 100 days without any symptom exhibiting itself. Thus, in this category, in which every circumstance favorable to infection was secured no less than one-third of the animals escaped, without undergoing any treatment whatever.

Examining the experience of other clinical professors, M. Renault says, that Professor Rey, of Lyons, has found that among animals who were accidentally bitten in the streets, and then placed under *surveillance*, 1 to 5 of the dogs, and 1 to 4 of the horses, became mad. Of those bitten or inoculated experimentally, 2 to 3 became mad. Of 16 animals accidentally bitten at Toulouse, 5 became mad. Professor Hertwig, of Berlin, states, that of 137 dogs accidentally bitten, 16 only became mad, 121 remained uninjured; while of 25 experimentally infected 10 became mad, and 15 did not. Thus, in both the categories, the proportion affected was sensibly less in Berlin than in France, whether this be owing to climate or other cause.

Thus, taking things at the worst, it results from these observations, made at different epochs, that two-thirds of the animals accidentally bitten, and one-third of those artificially infected, escape.

Still, it must not be supposed that these *mean results* of a great number of observations can be applied when speaking of the bite of a particular dog; for M. Renault has repeatedly observed, that while one dog, evidently mad, bites several others, and only one-sixth or one-seventh of these shall suffer, the virus conveyed from another dog, to all appearance in just the same condition, will infect nearly every animal (five-sixths or six-sevenths.)

Moreover, it is generally believed that the bites of *mad wolves* are oftener followed by hydrophobia than are those of dogs. Of 254 examples of such bites, the histories of which M. Renault has been able to collect in authors, in 164, or nearly two-thirds, hydrophobia followed—the proportion for accidental bites by mad dogs being only one-third. Whether this depends upon the fact, that in the wolf the rabies is oftener spontaneous, or upon the more remarkable one, that this animal almost always bites its victim in the face, neck, or head, is uncertain.—*Bull. de l'Acad.*

[Hydrophobia has recently been very prevalent in France, so that something like a panic has been produced and the journals teem with accounts of supposed remedies. The police have made a terrible onslaught on all wandering dogs; and the exemption of London from the disease, since the prohibition of the employment of dogs as beasts of burden, has been adverted to. It would be desirable to ascertain how far this exemption has extended beyond the metropolis, where alone the dog-act is in operation. How much remains to be cleared up in the history of *rabies canina*, is seen by a communication from Clot-Bey (*L'Union Med.* 1852, No. 94,) in which he states, that while dogs exist in such vast numbers in various parts of Asia and Africa, hydrophobia is of the rarest occurrence. In Egypt where he lived during twenty-five years, he never heard of a case, although the dogs wander abont the towns and villages in vast numbers, under a burning sun panting for breath, ill-fed, and often deprived of water. He thinks that much may be due to the promiscuous intercourse which their entire liberty allows to these animals. In Greece, on the other hand, the disease is excessively prevalent.—*British and Foreign Medico-Chirurgical Rev.*

On the Induction of Abortion in the Vomiting of Pregnant Women. By MM. Dubois and Stoltz.—During a recent discussion at the *Academie de Medicine*, M. P. Dubois stated the results of his experience in relation to obstinate vomiting in pregnancy. In proof that this is oftener a more dangerous occurrence than is usually supposed, he stated, that in the course of thirteen years he had met with twen-

ty cases in which it has proved fatal. That obstinate vomiting is but the exaggeration of the natural sympathetic vomiting of pregnancy, and not due to any special lesion, is proved by the facts, that at the autopsies nothing is found, and that when the process of gestation becomes arrested, whether spontaneously or artifically, the vomiting is ordinarily put an end to, although the woman may not be delivered until several days after, of a dead child, and may yet die of the effects of what she has already undergone. M. Dubois refers to several cases in which the women, apparently at the point of death, were saved by the *spontaneous* death of the fœtus, this being expelled only sometime afterwards. In respect to the question of how far *artificial* interference is attended with the same result, he furnishes notes of the four cases in which he has employed it. Three of these died and one recovered—this last being added to other cases on record, making the number of recoveries he is aware of certainly 7, and probably 9. In all the cases, however, whether fortunate or not, the vomiting was suspended by the operation. The difficulty is, indeed, to fix the *period* at which this should be resorted to; for it is the natural desire to delay this as long as possible, which leads to the fatal result—the woman dying, in fact, from the exhaustion and prolonged abstinence which the vomiting has induced, prior to the operations for arresting it being undertaken. M. Dubois lays it down as a rule, never to perform it when the signs of extreme exhaustion are present, as evidenced by considerable loss of vision, cephalalgia, comatose somnolence, and disorder of the intellectual faculties. On the other hand, we should also abstain from operating when the vomiting, though violent and frequent, still allows of some aliment being retained; when the patient, though wasted and feeble, is not obliged to keep her bed; when the suffering has not yet induced intense and continuous febrile action; and when other means still remain untried. In the first case, we should not save our patient, but perhaps accelerate her death, and bring discredit on the operation; while, in the other, we should sacrifice a pregnancy that might have gone on to the full time. It is, therefore, the intermediate period that should be chosen, and this is characterized by the following signs:—1. Almost incessant vomiting, by which all ali-

mentary substances, and sometimes the smallest drop of water, are rejected.—2. Wasting and debility, which condemn the patient to absolute rest.—3. Syncope, brought on by the least movement, or mental emotion.—4. A marked change in the features.—5. Severe and continuous febrile action.—6. An excessive and penetrating acidity of the breath.—7. The failure of all other means. But even within this period, which is of variable duration, the opportune moment must be chosen. This seems to have arrived, when the inefficacy of the most approved treatment has been proved, when fever is found to persist, and the debility and wasting of the patient are making sensible progress. The attendant should now declare that the operation is indicated, leaving to the patient and her friends the duty of deciding upon its adoption.

Professor Stoltz, of Strasburg, has published a highly interesting communication upon this subject, in which he also states his belief, that vomiting during pregnancy is much oftener fatal than is usually supposed. He relates four cases, from among others, that have come under his own notice. In three of these, death occurred, and life was saved by the operation in the fourth, although the case seemed hopeless. M. Stoltz lays great stress upon the operation being performed *in good time* because if we wait until the effects of the sympathetic reaction constitute in themselves a serious disease, the evacuation of the womb does not induce a cessation of these, and may, in certain cases, even hasten death—life, so to say, hanging upon a thread. It is undoubtedly difficult to say, *when* the moment has arrived that we can no longer hope for benefit from nature or therapeutical agents. But may not the same observation be made with regard to many important surgical operations? It is true that neither spontaneous nor artificial abortion always saves life in these cases; but the former usually occurs only when the woman's powers are hopelessly exhausted, and the pain and discharge consequent on the delivery may expedite her end—the same result not being unfrequently seen in fever. Some practitioners have expressed themselves very feelingly against sacrificing the child in these cases; but there is a great inconsistency on the part of those who do so, and who still advocate the operation in the case of narrow pelvis. A woman who has undergone artificial

abortion for obstinate vomiting, may hereafter (and these cases mostly occur in primiparæ) give birth to a living child, which can never be the case in one who has so narrow a pelvis as to call for the induction of abortion rather than of premature labor.—*Bull. de l'Acad. Gazette Med. —Ibid.*

On a Case of Poisoning from swallowing Chloroform, and on its Administration in Lead Colic. By Dr. Aran.—This occurred in a case of severe lead colic, for which the chloroform was given internally with advantage, when on the eighth day of treatment, the patient by mistake drank a draught from the bottle containing the chloroform. The burning sensation produced informing him of his error, he drank large quantities of water, and made ineffectual attempts to vomit. He was found a few minutes afterwards with his eyes shining, his features animated, singing and talking incoherently, and unable to recognize those about him. There were various convulsive movements, and the skin was insensible to pinching, pricking, &c. The pupils acted naturally, but the power of vision seemed gone. Pulse between seventy and eighty. In twenty or thirty minutes he fell into a sleep, which became very deep, and was accompanied by anæsthesia of surface and complete relaxation of limbs, the pupils and respiration continuing normal. Leeches were applied behind the ears, and purgative enemata given, and in a few hours he rose like a drunken man, to fall into a sound sleep again. Next day, he could remember nothing of what occurred, and the ill effects gradually wore off. As near as could be guessed, the man must have drank between eight and ten drachms of chloroform; and the innocuity of so large a dose can only be explained by the rapid elimination which Snow and others have proved that chloroform undergoes. M. Aran thinks that in similar cases, a preferable treatment would be the administration of strong coffee, and the application of cold to the head and sinapisms to the feet—means whose efficacy is known in cases of poisoning by alcohol, opium, ether, &c.

M. Aran speaks in warm terms of the internal use of chloroform in hysteria, spasmodic colic, lead colic, &c.,

given in increasing doses from 20 to 150 drops in the twenty-four hours. Not only has no accident occurred to any of the very numerous cases in which he has given it, but the physiological effects have at most in some cases resembled the transient intoxication induced by champagne. He has compared its efficacy in *lead colic* with that of the usual remedies, especially purgatives and alum, in a great number of cases. He does not deny the efficacy of *purgatives*, and thinks that they should be always employed in conjunction; but when given alone, only very powerful ones are efficacious, and then only temporarily so. He thinks even less favorably of *alum*, as he has found it very slow in operation, and of doubtful efficacy, beyond enabling mild aperients to act where drastics would otherwise have been required. *Chloroform* acts as opium or belladonna, by relieving the spasm, which constitutes so important an element in the disease. It has this advantage, that it may be given in considerable doses, without the incessant watching required by the other two, owing to its rapid elimination. It should not, any more than they, be employed alone, but in conjunction with means for ridding the economy of the lead, as sulphur and vapour baths, &c. The chloroform is to be applied locally, and administered in a mixture, in doses of from fifteen to twenty drops morning and evening, and as much in a glyster, increasing this quantity if required.—*Bull. de Therap.*—*Ibid.*

THE WESTERN JOURNAL.

THIRD SERIES.—VOL. X.—No. VI.

LOUISVILLE, DECEMBER 1, 1852.

UNIVERSITY OF LOUISVILLE.

The number ef Medical students in the University of Louisville is larger, as we go to press to-day, than it was on the corresponding day of last December, so that there is not likely to be any falling off in the class. The number, last session, was 261. Professors Flint and Palmer have fully sustained the reputation which they had acquired at the North as impressive and instructive teachers, and amply justified the choice of the judicious Board of Trustees. If delicacy permitted, we would like to enlarge upon the merits of our colleagues as lecturers, but we feel that we may safely leave their reputation with their pupils.

During the winter, it is the intention of the Faculty to issue a Catalogue of the students from the commencement of the institution, copies of which will be forwarded to the alumni or others desirous of possessing it, on application to the Dean.

We are pleased to learn that our friends in the Law Department have an unusually large class. Talents, attainments, and efforts to build up a first-rate school, such as theirs, ought to be successful, and it may safely be predicted will be.

OUR NEXT VOLUME.

Our Journal has now been in existence thirteen years. This number completes the 26th volume. It is the oldest medical journal in the West, and has been sustained so long that we cannot think of allowing it to come to an end, but on the contrary are making preparations to carry it on with renewed vigor. With our next issue we shall commence a new series, and hope to appear in a new type and upon paper of a better quality.

Our contributors also promise to give us more aid than they have done for sometime past. We do not like to make large promises, but we think we can assure our readers that they will find an improvement in our future numbers. We pledge ourselves to do all in our power to merit their approbation and patronage.

PROF. FLINT'S PRIZE ESSAY.

We have given up a large portion of the present number to the Prize Essay of Prof. Flint, assured that we could not devote it to matter more interesting to our readers. We need not bespeak for it their careful perusal; it may be be read again and again with advantage. The author will be found to have extended the boundaries of our knowledge in reference to this important subject.

THE DEATH OF DR. DRAKE.

Our readers will receive with deep sensation the intelligence of the death of Dr. Daniel Drake, long familiarly known to them as Senior Editor of this Journal. He died in Cincinnati, at 6 o'clock, on the evening of the 5th of Nov., of an affection of the brain, which he had been accustomed

to regard as of a congestive character. He was just about entering upon a course of lectures in the Medical College of Ohio, when attacked by the disease which terminated his useful life.

Dr. Drake was one of the pioneers of Medicine in the West, and one of the great men of the American medical profession. He was industrious, energetic, persevering, active, observant, and original. Of all the men with whom it has been our fortune to live on terms of intimacy, our impression is that he possessed the best mind—the most acute, analytical and comprehensive. He has left behind him, though in an unfinished condition, a work which, in our opinion, ranks first as an intellectual effort among the productions of American physicians. We trust that the second volume was so far advanced that his literary executors will have no trouble in completing it.

Dr. Drake had just entered his 68th year. He visited his friends in Louisville during the late session of the Kentucky Medical Society, and was here on the return of his 67th birth-day. His health appeared to be perfect, and no one could perceive any abatement of that clearness and strength of mind and activity of body for which he was so remarkable. As he was of a long-lived family, he had the prospect of a lengthened green old age before him. It is supposed that the anxiety and effort incident to the opening of the College to which he had returned so often, and concerning the success of which he had felt so keen a solicitude, had something to do in bringing on the fatal affection of his brain. It had long been his wish, often expressed, that he might die in Cincinnati and be followed to his grave by a large body of medical students and practitioners. His wish was gratified—and his friends have the consolation, not only that he lived long and usefully, but that his last hours were cheered by the hopes of the Christian.

Dr. Drake's life was eventful beyond that of most medical men. He was at once a laborious practitioner, a prominent public teacher, and a distinguished author. Entering into the practice of physic after attending one course of lectures, he had written a work which bore his name far beyond the boundaries of his own country before he returned to the University to graduate; and he had hardly taken his degree before he became a public teacher. He was a member of the first Medical Faculty ever organized in the Valley of the Mississippi, and took part in the first course of lectures; and his name is attached to the diploma of the first doctor ever graduated in the west. He was the first medical student in Cincinnati, and the first medical author.

Quitting the *Lexington Medical School*, after a single session, he returned to Cincinnati and procured a charter for the *Medical College of Ohio*, organized it, and in the fall of 1819 commenced a course of lectures in it. In the spring, as President of the Faculty, which filled and vacated its own board, he was dismissed by his two colleagues, one making the motion and the other seconding it, he in the chair putting the question. In 1823 he was again in Lexington lecturing with wonderful animation on Materia Medica in *Transylvania University*. Two years afterwards he was translated to the chair of Theory and Practice, and after two courses of lectures on that branch resigned his professorship and returned to his practice in Cincinnati.

In 1830 he was elected Professor of Theory and Practice in the *Jefferson Medical College* of Philadelphia, where he delivered a single course of lectures, resigning in the spring. The year following he organized the *Miami Medical College*, but a union of the new school with the Ohio Medical College having been brought about, he delivered a course of lectures on Clinical Medicine in the latter institution. In the spring he resigned. A few years later, he organized the *Cincinnati Medical College*. In 1839 he was invited to the *Medical Institute of Louisville*, where he labored until the spring of 1849, when he resigned his chair. The next winter he was found in his old chair in the *Medical College of Ohio*, from which he had been so unwisely expelled thirty years before. In the spring he again resigned; in the autumn he was recalled to Louisville, and after two courses returned to the Medical College of Ohio once more, where his eventful career has closed.

It would be with us a labor of love to write an eulogy upon our departed friend, but from the subjoined proceedings of the Medical Faculty of the University of Louisville, it will be seen that this task has been committed to one whose association with the deceased was longer and more intimate than ours.

At an early hour on Saturday morning, November 6, 1852, the Dean of the Medical Faculty of the University of Louisville announced to the medical class the melancholy intelligence of the death of Dr. Daniel Drake, so long identified with the University as Professor of the the Theory and Practice of Medicine, and late Professor of the Corresponding Chair in the Medical College of Ohio. As a mark of respect to the deceased, all further public exercises at the College were suspended until Monday. At a meeting of the Faculty at 5 o'clock, P. M., the following resolutions, offered by Prof. Gross, were unanimously adopted:

WHEREAS, it has pleased the Almighty Disposer of Events to remove from the scene of his earthly labors and the field of his usefulness and renown, Dr. Daniel Drake, Professor of Medicine in the Medical College of Ohio, and for many years a member of the Faculty of the University of Louisville, and whereas the deceased was greatly endeared to us by his many ennobling qualities, personal, social, and professional, by the great purity and uprightness of his life, and by his extraordinary mental endowments, his profound and various learning, and his zealous and untiring industry; therefore—

Resolved, That we have received with unfeigned sorrow the melancholy intelligence of the death of our late friend and colleague, who for so many years held and adorned the Chair of Theory and Practice of Medicine in this University, and who contributed so much to its elevation and usefulness.

Resolved, That in the death of Dr. Drake the American medical profession has lost one of its brightest ornaments, medical science one of its most zealous cultivators, and medical philosophy one of its most successful interpreters.

Resolved, That we will cherish his memory with affectionate regard and reverence, and that we acknowledge with the liveliest gratitude the debt which our profession and country owe to his labors and services.

Resolved, That this Faculty tender to the family of the deceased their heartfelt sympathy, and that the Dean be requested to transmit to them a copy of these resolutions.

Resolved, That these proceedings be published in the daily papers of the city, and in the Western Journal of Medicine, of which the deceased was for many years a distinguished editor.

On motion of Prof. Silliman, it was further—

Resolved, That Prof. Gross be requested to prepare a sketch of the life and services of Dr. Drake, as a memorial of the affectionate regard of this Faculty for a great and good man, and that the same be pronounced, if practicable, on some convenient occasion during the present session of this institution, before its officers and students.

L. P. YANDELL, Dean.

Since the foregoing was written, the following "Card" has been received, from which our readers will learn that the second volume of Dr. Drake's invaluable work will appear without any avoidable delay:

A CARD.—The undersigned desires to make known to the medical

profession throughout the country, that the late Dr. Daniel Drake had prepared a considerable portion of the second volume of his work on the Diseases of the Interior Valley of North America; the printing of which it was his expectation to have commenced early in the ensuing year.

It is the desire and intention of his children that the portion so prepared should not be lost to the Profession. It will be immediately revised by a competent person, and put, as nearly as practicable, in a shape to make it, as far as he wrote it, available to the public. As soon as this is accomplished it will be published.

CHAS. D. DRAKE, of St. Louis.

NOVEMBER 18, 1855.

BOOKS RECEIVED.

"Simon's General Pathology" has been issued by Blanchard & Lea in their usual neat and tasteful style, and a copy of the work has been laid on our table. We shall hasten to give our readers some account of its valuable contents.

We are indebted to Lippincott, Grambo, & Co. for a superb work—the "*Operative Surgery*" of Henry H. Smith, M. D.—a work which, we feel very sure, is destined to take a high rank upon the writings on Surgery. In fulness of detail and elegance of execution it will compare with any volume yet issued from the American medical press.

PRIZE ESSAYS FOR 1853.

It will be seen from the following circular that two prizes of the value of $100 each are to be awarded by the American Medical Association at its next annual meeting, to the two best essays that may be offered.

American Medical Association.—At the meeting of the Association held at Richmond, Va., May 1852, the undersigned were appointed a committee to receive voluntary communications on medical subjects, and to award two prizes of $100 each to the authors of the best two essays.

Each communication must be accompanied by a sealed packet, containing the name of the author, which will be opened only in the case of the successful competitors. Unsuccessful communications will be returned on application after June 1st, 1853.

Communications must be addressed post-paid, to the chairman of the committee, Dr. Joseph M. Smith, 56 Bleecker street, New York, on or before the 20th of March, 1853.

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